ANNALS

of the

Association of American Geographers

Volume XXX DECEMBER, 1940 No. 4

Symposium: Walther Penck's Contribution to Geomorphology*

O. D. VON ENGELN

When Secretary James asked me to organize a symposium on the general subject of Walther Penck's contribution to geomorphology and its relation to the Davisian system, I addressed a number of the persons who might be expected to manifest interest in such a conference and very shortly got enough favorable replies to warrant undertaking the project. Several papers were promised at once and eventually the program below was made up and presented as listed.

Walther Penck's Contribution to Geomorphology
Arranged by O. D. von Engeln

Introductory Remarks by O. D. von Engeln (5 minutes)
George D. Hubbard, Oberlin College
Major Objectives of Penck and of Davis in Geomorphic Studies (15 minutes)
Hellmut de Terra, Carnegie Institution (Introduced by D. W. Johnson)
Some Critical Remarks Regarding Walther Penck's Theory of Piedmont Benchlands in Mobile Mountain Belts (20 minutes)
Howard A. Meyerhoff, Smith College
Migration of Erosional Surfaces (20 minutes)
Kirk Bryan, Harvard University
The Retreat of Slopes (20 minutes)

* Papers and Proceedings of a session on this subject held during the Chicago meeting of the Association, December, 1939.

O. D. von Engeln, Cornell University
A Particular Case of Knickpunkte (15 minutes)
Walter Hacker, San Francisco State College (Introduced by John Leighly)
Overloading as a Motor of Mass Movements (10 minutes)

In order that prospective contributors, either of papers or to the discussion, should have something definite to consider, and to serve as a sort of probe of opinion on this topic, I prepared the Propositions which follow. These were sent to the members of the Association in mimeographed form. There was a very animated response. The replies received constitute a significant part of the symposium results. They provide a background for the papers and the discussion at the meeting.

It was disappointing not to have any addition to the propositions. The writer is not at all confident that he covered the whole field, and would have welcomed formulations on phases about which he was not informed.

There was adverse comment on the controversial form in which the propositions were framed. It was regarded inappropriate to present Penck and Davis as the chief protagonists of two opposed systems. The writer here disclaims any intention of fomenting acrimony in so doing, or of taking sides in a controversy. The purpose, rather, was to present the two approaches in sharp contrast in order that the differing viewpoints and objectives should be clearly revealed. It may also be recalled that Penck described Davis's uplift, still-stand, degradation cycle as a special-case scheme, and that Davis vehemently objected to such characterization. Further that Davis elaborately refuted Penck's accelerated uplift, piedmont bench-lands formulation. Although, as has been pointed out, truth resides in both approaches, their several sponsors recognized and gave free expression to the element of conflict. It is, hence, not a reflection on the scientific memory of either to present the issues they raised as debatable questions.

THE PROPOSITIONS AND THE RESPONSES TO THEM

In this section the propositions are reproduced and then the replies received. Informal replies were returned to the authors for revision before insertion herein for publication.

Proposed Symposium
on
W. Penckian *vs.* Davisian Geomorphic Tenets

There follow a number of formulations presenting what seem to be the leading differences in objectives, principles, approaches, and viewpoints of the two schools. You are asked to comment on these for purposes of clear-

ing the ground before the symposium occurs. Comment by (a) correcting errors, (b) rephrasing for greater accuracy, (c) eliminating items not *apropos,* (d) adding new items of significance. By this means it is hoped to have the discussion center on topics of greatest interest and pertinence.

Proposition 1. Davis conceived the major objective of geomorphic studies to be the recognition of stages in the systematic evolution of landforms as conditioned by the nature of the cycle, by structure and by process.

W. Penck conceived that land forms are primarily a reflection of the manner (progression) of uplift and considered the ultimate goal of geomorphic science to be the reading of the epeirogenic and orogenic history of the earth's surface relief, and from this reading to derive clues in regard to the nature of diastrophic forces.

Question: How may these fundamental differences in objectives, and the differences in approach they entail, be reconciled? Should such reconciliation be attempted? Does acceptance of either invalidate the other?

Proposition 2. Davis and his English and American predecessors either formally or tacitly assumed that diastrophic movement is "normally" rapid in rate and, even if recurrent, occupies only a small fraction of geomorphic time. He recognized other possibilities but considered them aberrant, non-representative conditions and occurrences.

W. Penck regarded continuously accelerated uplift to culmination and cessation, involving an expanding area and requiring extended geomorphic time for its consummation to be the representative case. He frowned especially upon intermittent, relatively instantaneous uplift separated by long pauses free of diastrophic change.

Question: To which of these opposed assumptions does the accumulated fund of geologic and geomorphic science lend the greater support? What evidence may be cited in behalf of the one or the other?

Proposition 3. Davis makes structural variation the fundamental condition governing the forms and sequence of the fluvial geomorphic cycle.

W. Penck makes homogeneity of structure a basic assumption and seeks to relate, with escape of drainage to the sea, all further development of surface relief to the interaction between processes of weathering and stream erosion, on the one hand, and the circumstances of uplift on the other.

Question: Is structure the dominant factor, as Davis holds, or subordinate, as W. Penck's approaches would make it?

Proposition 4. Davis ascribes all breaks (*Knickpunkte*) in the longitudinal profile of streams either to (a) alternations of resistance of beds, etc., in an ungraded course or to (b) intermittent uplift.

W. Penck contends that such Knickpunkte are normal to drainage development by radial streams over the slopes of an expanding (in area) dome

of homogeneous rock experiencing continuously accelerated (but uninterrupted) uplift.

Question: Are Knickpunkte exhibiting upstream migration possible (*i.e.*, can such be maintained in the profile of a stream) except as the course is (1) ungraded (2) has beds of alternating degree of resistance to erosion?

If so, is intermittent uplift a condition essential to their development and persistence?

Proposition 5. Davis held that piedmont bench lands in homogeneous structure are evidential of intermittent uplift, each bench representing an erosion level developed during a considerable period of still-stand.

W. Penck held that piedmont bench lands are a normal expectancy with continuously accelerated uplift involving an expanding area, their steep fronts being the interstream correlatives of Knickpunkte.

Question: If W. Penck's concept of bench lands is held not valid, are the rest of his formulations also rendered inapplicable?

Proposition 6. Davis conceived that the degree of valley slope declivity was primarily a reflection of stage in the progress of the fluvial geomorphic cycle. Also, that great irregularities in the stage of youth were conditioned by interactions between structure, rate and manner of stream erosion, varying weathering processes.

W. Penck held that, assuming homogeneous structure, the profile of slopes, convex, plane, concave, is determined by the circumstances of the uplifting action. If the rate of uplift is constantly accelerated, slopes will be convex; if uniform, plane; if declining, concave.

Question: Does this difference in appreciation and interpretation of slopes come from a failure to distinguish between degree of declivity of slopes and form profile of slopes? If both elements are significant what interpretation is to be given each?

Proposition 7. Davis held that once grade profile was established by streams and slopes were graded also, the further history of slopes was contained in a progressive decline in their steepness.

W. Penck maintained that once established with reference to an unvarying altitude of base the further history of slopes was retreat in planes parallel to the original degree of declivity, and extinction when this plane is finally intercepted along divide lines by the progressive growth and rise of *Haldenhänge*.

Question: Does the concept of parallel retreat of slopes have verity, if so under what conditions? How precisely shall *Haldenhang* be translated? Literally it is 'waste slope'. But that would be no more than talus slope, for which a word exists in German, namely *Schutthalde*. Haldenhang is evidently a word coined by W. Penck. What was the term meant to designate?

(A) What of the above is misconceived or misunderstood?
(B) What of importance has been omitted?

The great variance in opinion indicated by the responses to this outline are best grasped by having all the replies to a given question appear together. But this method of presentation would do injustice to the several authors, each of whom would no doubt regard its continuity an important part of his contribution. The author sequence is, therefore, followed here. However readers who are interested specifically in any one of the issues raised will find it profitable to collect all the replies on that point for direct comparison.

Two comprehensive replies were received. These are presented first because they are thus inclusive and also because they are representative of the sharp difference in attitude that exists with reference to the subject as a whole.

COMMENTS BY JOHN LEIGHLY, UNIVERSITY OF CALIFORNIA

Herewith are my replies to the questions posed in the circular letter "Proposed Symposium on W. Penckian *vs.* Davisian Geomorphic Tenets."

First of all, I do not exactly like the placing of Penck and Davis in alleged opposition, in the title of the symposium and in the introductory statement. We are not concerned here with two dogmatic systems of thought that are held firmly as wholes or that are mutually exclusive. It may be taken for granted that a complete theory of geomorphic development is possible. It may be further granted that such a theory does not exist at present, but that already we have, from observation and reasoning, a part of it. From the fragment that observation has provided, various efforts have been made to construct the whole. For the purpose of this symposium two of these efforts have been selected. One system, that of W. M. Davis, is the more comprehensive, pretending to provide a frame into which all examples of geomorphic development may be fitted. Any such pretense, in the present state of our knowledge of the processes involved, is to be viewed with skepticism: sympathetic skepticism to be sure, but skepticism nevertheless. Along some lines our knowledge is greater than what Davis had at his disposal when, say thirty years ago, he had essentially completed the formulation of his system. Sympathetic skepticism was, therefore, even more in order then. The other systematic structure cited, that of Walther Penck, is, by the admission of the sub-title of the principal work in which it is presented, a fragment. A sufficiently large fragment, however, to constitute nearly an entire system by itself. The different points of view of the two men, and also their different mental sets, make the two systems conflict in part.

It would seem that the proper purpose of a parallel discussion of the two systems would be to map out the areas of conflict, and within these areas to discuss the character of the conflict and test the competing ideas against observation and deduction. In both systems, moreover, there are large areas of deduction. This deduction may be checked for accuracy of premises and conclusions. Undoubtedly errors in reasoning may be found in both, and they can be eliminated. I think that a thorough examination of the two systems will lead to the discovery of useful ideas in both, and in both ideas that should be rejected. Probably Penck's account will yield more of value than Davis's, because Penck proceeded more carefully from a consideration of processes. But since much in the way of process can not easily be observed, and since fragmentary observations may lead to wrong conclusions, some of Penck's account is probably wrong. The ideal, complete theory of the future will include part of Penck and part of Davis; but not all of either.

Proposition 1. Davis worked toward an understanding of land forms as an end in itself. Since land forms are produced in time—that is, since they are not produced instantaneously, but undergo development—it seemed to Davis essential that we be able to recognize where in the chain of development any given example falls. It is to be noted that since the eighteenth century the setting up of a developmental series of phenomena has been a recognized method of ordering facts for which a mechanical explanation is unavailable. Witness Schelling in philosophy, and, among pre-Davisian natural scientists, Playfair, Lyell, and Darwin. Davis conceived the universe of possible land forms as a time sequence; hence the essence of a characterization of a particular form or set of forms was to him the placing of the form or set of forms in its appropriate place in the time sequence. "Stage" therefore became the essential quality to be recognized. Penck, in the *Morphologische Analyse* at any rate, was not considering land forms, but rather the diastrophic history of the land, as the central object of investigation. He undertook to use morphologic data toward the end of elucidating this diastrophic history. In order to use these data for that purpose, he had to develop a theory of the relation of form to diastrophic process, and, since the forms produced directly by diastrophism are constantly undergoing modification by the exogene forces, he had to work out a scheme whereby the effects of diastrophism may be distinguished in the landscape from the effects of denudation and erosion. The essence of this scheme is the theory of development of slopes under different degrees of equilibrium or disequilibrium between diastrophism and denudation. So thoroughly did Penck do this job that, though it was intended only as a part of a larger whole, it forms a fairly well rounded structure by itself. It is to be noted,

however, that it covers only one phase of the shaping of land forms: the sculpturing of the land surface by the degradation of slopes tributary to stream courses. There is a vast range of geomorphology left out of the discussion: the degradational work of glaciers, winds, and waves, and the depositional forms produced by all agents. Penck produced a masterly discussion of one set of processes, that enters as a constituent into the general lore of geomorphology, but that is very far from being, in intention or execution, a general geomorphology such as Davis conceived.

The great forward step beyond Davis that Penck took is not to be comprehended as a system that replaces even a part of Davis's work; it is rather an exemplification of the procedure that will lead geomorphology beyond the point where Davis left it. This procedure consists in close attention to processes of translocation of weathered rock material. Penck discussed, essentially, only one set of processes, namely those associated with the development of slopes; but the same kind of attention to process will remake the rest of geomorphologic science. Davis's great mistake was the assumption that we know the processes involved in the development of land forms. We don't; and until we do we shall be ignorant of the general course of their development. In his eagerness to set up a general system, Davis jumped over the preliminary, necessarily painfully slow study of processes, and so left his system with an inadequate foundation. When this foundation is finally laid, it will probably be found that Davis's superstructure will have to be modified greatly in order to fit it.

Proposition 2. We know little about the relative rates of diastrophic and degradational processes. The diastrophic movements we are able to observe are necessarily the rapid ones. Direct observation, therefore, leads us to ascribe a greater rate of change of elevation to diastrophism than to degradation. But there is no reason to think that rates of diastrophic changes in elevation do not vary within wide limits. It would appear desirable, therefore, to admit, as Penck does, the possibility that rate of degradation may have any one of a wide range of ratios to rate of uplift by diastrophism. At one extreme, in uplift so rapid in relation to rate of degradation that it may be considered instantaneous, we have the condition from which Davis deduced his cycle; at the other, uplift so slow in relation to degradation that no net rise of the surface is effected; this latter case gives the *Primärrumpf,* one of Penck's magnificent generalizations. Between these two limits is a wide range of ratios; application of the criteria Penck sets up permits us to recognize which of the two, uplift or degradation, has the greater rate.

Proposition 3. I don't agree with the statement made concerning Penck's neglect of heterogeneity of structure. It was necessary for him to

develop first the theory of slope development in homogeneous material. Once that theory was worked out, no great amount of space needed to be used for application of the theory to slope development in heterogeneous materials (*Morphologische Analyse,* p. 136 ff.). All three of Davis's trilogy of factors, "structure, process, and stage," are more complex than Davis imagined; at least Davis presented them in his theoretical writings as simpler than they really are.

I don't know that anyone has pointed out that these three factors reappear under other names in Sauer's formulation of the development of landscape: agents working ("process") on the earth's surface ("structure") through time ("stage"). Such a formulation is perfectly in accord with Penck's reasoning, though Penck, like Sauer, and, I think, correctly, treated time as a homogeneous continuum instead of dividing it into arbitrary segments ("stages") as Davis did. Or instead of making rate of time variable, and changing the scale of time according to the resistance of the rocks of a region. This was a geologic dodge rather than a physical one. I know that if Penck's reasoning is applied to a region of complex structure—we constantly see it exemplified in an area of exceedingly complex structure, the California Coast Ranges—it does at least as much justice to structure as Davis's scheme ever did. It does a great deal more justice, in my opinion, for it enables one to see in one's mind the way in which structure gradually becomes evident in relief. Once on an automobile trip through the local section of the Coast Ranges, the Berkeley Hills, Davis disposed of the complicated series of forms to be observed here by a single phrase, "maturely dissected." Penck's reasoning makes rounded hilltops, rock bosses, spatulate canyon heads, and slightly incised stream channels "come alive" as forms in which the structure of the ranges is gradually becoming more and more evident in their relief.

Propositions 4 and 5. I can not see how Knickpunkte can be formed in slopes or longitudinal stream profiles in a homogeneous mass undergoing continuous uplift. I think Penck is in error here, though a discussion would require consideration of the large literature on *Piedmont-treppen,* most of which I haven't read. This error does not invalidate his general theme of slope development.

Proposition 6. I don't recall that Davis ever discussed slope profiles, even to the extent of being exercised, as G. K. Gilbert was, over the phenomenon of rounded hilltops. Penck's direction of attention to this quality of slopes is a permanently important contribution. Undoubtedly both declivity and profile are important; declivity, as a function of rate of valley cutting and rate of denudation of the slopes tributary to stream channels, receives its full due in Penck's reasoning. Profile in both heterogeneous

and homogeneous rocks acquires a significance that was apparently undreamed of in Davis's philosophy.

Proposition 7. The parallel retreat of slopes is probably not to be taken in an absolute sense. Between the two extremes of a slope on which the products of weathering are removed immediately, so that there is uniform exposure of the whole extent of the slope to weathering, and a slope so gentle that the mantle of weathered material provides a uniform protection of the underlying rock against weathering, there is an intermediate state in which the layer of weathered material becomes thicker downslope in such a manner that the upper parts of the slope are reduced more rapidly than the lower parts. Thus there are developed convex profiles independently of diastrophism, the convex profiles of rounded hilltops (*Morphologische Analyse,* pp. 115–117). Penck's ideal derivation is only two-dimensional; it refers only to transverse profiles of ridges and troughs composed of planes that intersect in parallel lines. Slope gradients across a profile lie in the same plane, a plane normal to the ridge crest. As soon as one admits surfaces, such as are commonly found in nature, in which slope gradients are variously oriented in space, multiplicity of ridge crests makes the areal extent of surfaces undergoing rounding greater as the number and linear extent of the ridge crests increase. Thus what Davis would have called a "maturely dissected" surface, in which ridge tops are rounded, might produce the effect of progressive flattening of slopes, since in earlier stages of dissection the area covered by rounded ridgetops is smaller. Davis's thesis of a progressive flattening of slopes was an illusion growing out of his disregard of what is actually happening on slopes in the way of weathering and transport.

As to the verity of the parallel retreat of slopes. The most perfect example I have seen is in the Bad Lands of South Dakota, where one can almost measure the angle between slope and valley floor (this is the apparent relation; in fact the angle is a *Haldenscheitel,* in Penck's terms) with a carpenter's try-square. This is the case referred to by Penck (*Morphologische Analyse,* p. 116, lines 6 to 10 from bottom of page), though it is not exactly *Abspülung* that removes the waste from the slope, but a liquefaction of the surface layer after it has become saturated, so that the liquefied layer flows off completely on to and across the valley floor (Haldenhang), which, because of the low viscosity of the sheet of waste, has a very low slope toward the drainage channel. Parallel retreat is well exemplified in the retreat of scarps in central Kentucky. Kirk Bryan has described it from the Southwest, as the extension into mountain masses of piedmont slopes. These examples, picked at random from areas I know by personal observation and among which there is much diversity of manner of denuda-

tion, certainly suffice to establish the reality of the process. It could certainly be observed in innumerable other places.

My translation of Haldenhang is "denudational slope." It is the rock slope undergoing recession, on which there is usually a blanket of weathered material migrating downslope, mainly by mass movement, toward a drainage channel. It is a slope of rock *in situ,* as contrasted with the slope of the upper surface of the blanket of migrating rock waste, which in a state of dry accumulation we call "talus slope." It is not the slope of the *Halde,* but the slope that yields the material of a Halde. I think "denudational slope" is probably a better term in English than Haldenhang is in German, since literally the German term suggests the slope of the upper surface of a Schutthalde.

MEMORANDUM BY DOUGLAS JOHNSON, COLUMBIA UNIVERSITY ON THE MIMEOGRAPHED OUTLINE OF THE PROPOSED SYMPOSIUM ON THE GEOMORPHIC IDEAS OF DAVIS AND WALTHER PENCK

(Explanatory Note: The opinions respecting Davis's ideas expressed in the following pages were gained in the course of a year of study in his seminar at Harvard, six additional years of very close association with him on the Harvard Faculty, and thirty years of intimate acquaintance during which a somewhat voluminous correspondence passed between us dealing with his ideas on a wide range of geomorphic concepts. In editing a volume of his essays, I had repeated occasion to discuss with him various aspects of his beliefs, particularly on matters in which contradictory points of view appeared to be set forth in certain of his essays. When I speak of Davis's views in the paragraphs that follow, I refer to his fully matured views as I came to understand them in the light of the associations, correspondence, and discussions mentioned above.

I can claim no such full acquaintance with the views of Walther Penck. I have found his text very difficult to read, and even more difficult to understand. Expert German scholars inform me they have had the same difficulty. All I can profess, therefore, is a general idea of certain major aspects of Penck's geomorphic concepts.)

Proposition 1. According to my understanding Davis conceived that the major objective of geomorphic studies was to form the basis for an effective geographic description of the earth's surface features. The recognition of stages in the systematic evolution of landforms was merely a means to this end, in his opinion the best possible means. If, as I believe, the major objective Penck had in view is correctly stated, I see no possibility of reconciling the observed difference in objectives. First, because the objectives are widely different; and secondly, because the objective Penck

apparently had in view is in my opinion utterly unobtainable. The history of the earth's surface relief is undoubtedly affected by the character and rate of land uplift or sea level depression; but since that history is also affected by many other factors such as structure, lithology, climate, etc., and in varying degrees in diverse ways under different conditions, it is quite impossible, in my judgment, to accept present surface form as a trustworthy record of one only of the numerous factors which have produced the observed result.

Proposition 2. For simplicity in exposition Davis usually assumed relatively rapid uplift of a land mass, with substantial preservation of its initial form, followed by sequential changes in an evolutionary series as a result of the operation of destructive forces upon the uplifted mass. He did not, so far as I know, definitely consider that such rapid uplift was "normal" and slow uplift "abnormal." He repeatedly pointed out that mountains or plateaus might be raised so slowly, or fault scarps elevated so gradually, that extensive erosion would profoundly dissect them long before the movement had ceased.

The fact that Davis, for pedagogical reasons, so frequently made certain assumptions which aided a clear presentation of the successive stages through which an uplifted land mass would pass, has led many to believe that he did not recognize the possibility of slow uplift with concomitant extensive erosion. He recognized not only the possibility, but also the probability that this latter situation obtained repeatedly in the past. He also recognized that high mountain ranges little dissected testify to relatively rapid uplift during periods so short that erosion accomplished relatively little before observed altitudes were attained. It is possible that he believed this latter situation more frequent than the former, and one may fairly draw this inference from certain of his writings. On the other hand, he at times emphasized so strongly the probability of extensive erosion during slow uplift that I do not feel at all certain he would be willing, were he passing thoughtful judgment on the matter, to characterize one set of conditions as frequent and normal, the other as rare and abnormal.

Professor Davis did regard Penck's conception of a progressively accelerated dome-like uplift over a constantly expanding area as highly theoretical and artificial, and as too rigidly restricted to be in any sense truly representative of land uplifts in general. In this opinion I fully share. It seems to me that the accumulated fund of geologic and geomorphic evidence abundantly establishes the existence of countless uplifts of a totally different shape and character, and demonstrates the frequency with which uplift occurs so rapidly that erosion has accomplished relatively little before high altitudes have been attained. In general I would say that the bulk of geo-

logic evidence associated with block mountains, young plateaus, dissected peneplanes, and certain other types of land forms, favors the conception that land uplift is frequently intermittent and at times relatively rapid, separated by long periods of comparative freedom from diastrophic change. I know of no satisfactory evidence in support of the highly speculative type of uplift postulated by Penck.

Proposition 3. If I correctly understand Davis's writings and oral teachings, he did not make structural variation the fundamental condition governing the forms and sequence of the fluvial geomorphic cycle. He did regard structure as one of several factors controlling the evolution of land forms, and he recognized that structure sometimes plays a dominant rôle in *the cycle of land-mass dissection.* But when he wished to illustrate the normal development of *the fluvial cycle,* he usually omitted structure from his discussions, and avoided showing any structure in his diagrams of stream development. It is essential that a clear distinction be made between the cycle of land-mass evolution, and the cycle of the thing which is dissecting the land-mass. There is no necessary relation between the two. A fluvial cycle will run through the same sequence of stages whether the stream be flowing on resistant or non-resistant rocks. If geological structures are such that the developing stream is located here on resistant rock, there on non-resistant rock, the different parts of the stream will develop at different rates. But the fluvial cycle will pass through the same sequential stages in either case. Thus structure can be, and usually is, ignored in discussions of the fluvial cycle. Structure is important, and may become dominant, in determining the detailed forms of the cycle of land-mass evolution.

It seems to me quite proper for Penck to assume homogeneity of structure in outlining his conception of the cycle of land-mass development under stream dissection, just as Davis assumed homogeneity of structure, or ignored structure, in outlining his conception of the development of the fluvial cycle. I do not understand that Penck would exclude the influence of structure on details of form in observed specific instances of land-masses undergoing dissection. It seems to me, therefore, that the question under Proposition 3 is based on a misconception of Davis's ideas and perhaps also of Penck's.

Proposition 4. It is not my understanding that Davis ascribes breaks in the longitudinal profiles of a stream to one of the two causes stated under (a) or (b), namely, alternations of resistance of beds, etc., and intermittent uplift. Of course one cannot tell what is intended to be included in the expression "etc."; but Davis recognized many causes for the development of breaks in stream profiles, of which the following are a few: Initial surface irregularities of the land-mass on which the stream first developed; glacia-

tion; stream capture; cutting off of meander spurs; drop of sea level; deposition of fans by tributary streams; uparching of the earth's crust and faulting, which need not be intermittent, although perhaps they were intended to be included in the expression "intermittent uplift."

Penck's contention that Knickpunkte are normally developed in the drainage by radial streams on an expanding dome of homogeneous rock experiencing continuously accelerated (but uninterrupted) uplift, seems to me to be based on wholly fallacious reasoning. The question at the end of this proposition seems to me so phrased that it is impossible to answer it, since by definition a stream is not graded if it has Knickpunkte scattered throughout its course. If there are beds of alternating degree of resistance involved in such an uplift as Penck conceives, Knickpunkte will develop, and the stream therefore will not be graded. The last part of the question, "Is intermittent uplift a condition essential to the development of Knickpunkte" can be answered in the negative. Intermittent uplift is one possible cause of Knickpunkte; but there are other causes, such as drops of sea level, differences in rock resistance, stream capture, etc. But some cause other than progressive uniform uplift seems to be required.

Proposition 5. Davis recognized that drops of sea level, as well as intermittent uplift of the land, would cause piedmont bench lands in homogeneous structure. Penck's conception that such bench lands can develop on a uniformly rising and expanding dome seems to me impossible of acceptance. The reasoning by which he accounts for the development under the conditions he assumes seems invalid. Critical analysis of the defects in his reasoning would require more time and space than is here available, but some of these defects are discussed by Baulig in a recent issue of the *Journal of Geomorphology*.[1]

The question as to whether the remainder of Penck's formulations are rendered inapplicable if his concept of bench lands is held invalid, is pretty sweeping. I think one could fairly say that all of his reasoning dependent on the basic erroneous conception involved in his statement of bench land development is invalid, and that other portions of his reasoning, some of which are discussed in this memorandum, are likewise invalid.

Proposition 6. The conception respecting valley slope declivity here ascribed to Davis seems to me correct. On the other hand Penck's conception that slope profiles are convex, plane, or concave, according to the circumstances of the uplifting action, is in my judgment one of the most fantastic errors ever introduced into geomorphology. One may in the same region, which has experienced the same history throughout (as for example in the Bad Lands of South Dakota or in certain portions of the Black

[1] Baulig, Henri, Sur les "Gradins de Piedmont." *Jour. Geomorph.*, II, 281–304, 1939.

Hills), photograph both convex and concave slopes within a short distance of each other.

In answer to the question I would say that the fundamental cause of Penck's error is his failure to realize that present valley slopes, whether convex, plane, or concave, are constantly changing expressions of temporary conditions of grade, and not in any sense permanent records of past rates of uplift. Since present conditions of grade are affected by uplift, lithology, structural relations of beds of varying resistance, climate, stage of land form development and other factors operating jointly to a given end, it is manifestly impossible to isolate one of these variables—the rate of uplift—and attribute the character of the slope to this one variable alone. In further answer to the question, it is equally impossible to explain all convex or concave slopes in terms of one or another variable, such as climate. There are both convex and concave slopes in both arid and humid regions. As a matter of fact, most valley walls are neither wholly convex nor wholly concave, but prevailingly convex above and concave below. Under special circumstances, however, one or more of the variable factors affecting slope may be dominant, and then it may be possible, in a given instance, to attribute convexity or concavity primarily to this or that cause. This is apparent in the Bad Lands region, where certain beds, or certain successions of beds, give slopes which are almost wholly convex; while another type of bed, or succession of beds, closely adjacent, give slopes which are almost wholly concave. Since widely separated areas almost always, if not always, differ in climate and other controlling factors, I doubt if it will ever be possible to isolate the factor of uplift and ascribe convex or concave slopes to the rate of uplift alone.

Proposition 7. In this proposition Davis's earlier conception of the history of valley slopes is set forth. Later he accepted the view that valley walls, like mountain fronts, might retreat parallel to their former declivities.

In this he was influenced by Lawson. Lawson states his conception, but does not critically analyze the reasons for such parallel retreat. I have never been able to accept his contention that parallel retreat will result from weathering of the mountain front, nor Davis's acceptance of a similar parallel retreat of valley walls from the same cause. Penck's analysis of the cause of parallel retreat seems to me unconvincing.

In answer to the question: "Does the concept of parallel retreat of slopes have verity?" I would reply in the affirmative. In my opinion the necessary conditions are basal sapping of the slope either by the main stream to which the slope is parallel (as in the case of valley walls), or by lateral streams issuing from the base of the slope and intermittently deflected right and left against the base of the slope by the deposits of these streams.

COMMENT BY GEO. D. HUBBARD, OBERLIN COLLEGE

Proposition 1. Here the difference is one of emphasis. Davis recognized diastrophism but I think Penck scarcely recognizes "stages." Yes, reconciliation is necessary and should be attempted. Acceptance of either objective does not invalidate the other. Davis once said to me, "I emphasize this or that because everyone else does not."

Proposition 2. I think the evidence indicates that more of our diastrophic movements have been decidedly intermittent, but aperiodic, long nearly quiet periods between shorter more active periods. Periods of mountain making between long periods of erosion, see more or less completed cycles of erosion and widespread unconformities. No doubt some diastrophic disturbances have been slow, even cumulative and culminative, as the depression of lands under ice load and the reverse; as slow rise of areas as they are unloaded by erosion.

Proposition 3. I cannot answer fully. I have seen structure enough to know that structure and strength are far from uniform and I feel that diversity of structure and resistance is more common than homogeneity.

Proposition 4. Intermittent uplift can produce Knickpunkte but so can alternating hard and soft layers. My experience is not broad enough to say which is more important, or more frequent, but I think the second.

Proposition 5. I do not see that a breakdown of Penck here, wholly invalidates all his interpretations though I think it is a rather good testing point.

Proposition 6. I believe most streams go through the convex to concave profile much as bluffs do, if strata are sufficiently homogeneous, and this in spite of uplift interruptions. I can see that Penck's doming would tend to give streams a convex profile, but if age can be reached it has to disappear.

Proposition 7. My observation is with Davis here. Retreat is not in parallel planes, if I know what Penck means.

I have never seen successive planes of advancing age, either in valley longitudinal profile or in valley slopes in cross section retreat in parallel forms. I can't see how either curve, bluff, or valley longitudinal can possibly be "successively parallel" with original or any later form.

COMMENT BY BAILEY WILLIS, STANFORD UNIVERSITY

I may say in advance that as an intimate associate of Davis and Penck I would think neither of them would agree entirely to the several propositions (numbers 1 to 7, inclusive) as statements of their views. Davis in particular derived his ideas from Powell and Gilbert, who always emphasized the intimate relation between the internal activity of the earth and the reaction

of the atmosphere in its various forms of sculpture.

Davis, it is true, emphasized the sculptured form but to a certain extent he recognized that warping of the earth's surface was an important antecedent as indicated by the term "consequent streams." There were differences of concept between Davis and Penck as to the forms and rate of deformation, but I would hardly have thought they were worthy of distinction as two schools.

I myself began my physiographic studies in 1885 under Gilbert and Powell and at that time independently reached the conclusion that the sculpture of the Appalachian region must have been reduced to a low plain in order that the rivers might have become adjusted to courses on anticlines in lieu of the postulated original courses on synclines. Since then I have regarded rivers as the most significant indices to the activity of deformation and in placing emphasis on them have diverged from the lines of research so ably followed by Davis.

It is very interesting to find that Penck's views have gained sufficient influence on American thought to be worthy of such a discussion.

Proposition 1. Davis's conception is stated as I understand it. Penck's also, but I would say that Penck was in error in denying the validity of the cycle concept and excluding time.

Proposition 2. The terms "rapid" and "geomorphic time" are here relative, each to the other, and the latter is unfortunately vague. The ideas would be more sharply defined if one said (as I have heard Davis put it) that the uplift of a mountain range is relatively rapid as compared with the duration of peneplaination; and that is, I think, the fact.

Penck's ideas concerning the acceleration of uplift of an expanding area appear to me to be highly theoretical and "rare-actual." The use of the word "instantaneous" is not consistent with any geological action, except an earthquake or explosion.

I would say that the progress of knowledge emphatically sustains Davis's views.

Proposition 3. I do not think Davis would agree that he regarded "structural variation" as the "fundamental" condition. While regarding it as a very important factor in the control of sculpture, he would not emphasize it in contrast to uplift, tilting, etc.

If Penck did hold that "homogeneity of structure" was the "basic" assumption, he simply contradicted the facts. I very much doubt that he did.

Proposition 4. Davis's views are sustained by the facts, as here stated, but he would not have excluded other conditions, such as the occurrence of hard beds or climatic changes.

Proposition 5. Again the statement of the proposition is so limited that it is half true, so far as Davis is concerned, but only half true, because so incomplete.

Penck's idea might apply in some special cases, but not generally.

Proposition 6. Davis's views are, in my judgment in accordance with the facts, including all the variable, natural controls.

Penck's again, as here paraphrased, are based upon a very special condition of uplift, which I have not seen in nature. They present a too common habit of mind, which lies in following a subjective concept and trying to interpret facts accordingly.

Proposition 7. If I rightly understand the meaning of the term grade-profile it implies that the incline (whether stream channel or valley side) has been reduced to that condition which precludes further transportation of sediment by running water. I don't see how there could be any further "decline in steepness." When the grade-profile has been established over an expanse, that surface has entered upon the stage of a peneplain, as I use the terms.

Wass W. Penck mit "Haldenhänge" gemeint hat, weiss ich nicht. Eine Halde ist a "hillside," oder "slope." Man kann daraus machen wass man will.

In general, I would suggest that in order to interpret or evaluate a man's writings it is necessary to know how he got his initial ideas. Penck was a German student and his scientific thinking was characteristic of that people, who have demonstrated great capacity for observation and research, along with a genius for fairytales. Davis was an anatomist by nature and his studies took that turn. He might be called the osteologist of geomorphology. But he had absorbed from Powell and Gilbert the broad, philosophic concepts of erosion and his generalizations were governed by them. See Powell in *Physiography of the United States* (American Book Company, 1896), pp. 1 and 10–11.

COMMENT BY GEORGE B. CRESSEY, SYRACUSE UNIVERSITY

The fundamental clash between Davis and Walther Penck concerns the flattening of slopes with time. Is there a cycle, in which valleys open by the progressive lessening of declivities, or do slopes preserve a characteristic angle from beginning to end governed by rock and climate and process? This concerns the valley side or wall (*Bösche*) rather than the floor (Haldenhang). Along with this are conflicting ideas as to the dynamic behavior of the earth; is uplift intermittent or continuous? Whereas Davis favored the former and used diastrophism to decipher the geomorphic history of an area, Penck considers land forms to be a clue to the increasing or decreasing

intensity of diastrophism.

Unfortunately, Americans and Germans have approached the problem from diverse areas. In the humid eastern United States climatic influences are different from those in German Southwest Africa. It is significant that when Davis in his later years worked in Arizona, his description of pediments and the steep face of retreating mountain walls is not so different from Penck's Haldenhang and Bösche. Soil creep in humid lands masks the distinct surfaces and angles of arid regions, and has kept us from understanding the processes more clearly revealed with aridity. Thus humid monadnock slopes preserve their steepness for reasons comparable to those of the arid inselberg. Each disappears by the inward retreat of its walls rather than by flattening and downward wasting. The very fact that monadnocks exist at all indicates that the erosion cycle proceeds by valley widening rather than slope flattening.

Penck's stimulating ideas have provided an arresting challenge to the complacency of American geomorphology. What we now need is the critical application of his ideas to land forms in the eastern United States. Our geomorphology has too long been content with stereotyped description.

COMMENT BY JOHN L. RICH, UNIVERSITY OF CINCINNATI

Proposition 1, 1st paragraph. "Doubtful of the major objective." Answers to questions: "No," "No."

Proposition 2, Question—support? "To the first—all the evidence points to different rates of uplift in different places and times—why expect either to be exclusively correct?"

Proposition 3, 1st paragraph. "Does he—How about normal pattern on uniform rocks?" Question. "Neither as usual—Wherever you find disagreement both sides are usually right in some circumstances, and neither side right in all.

Proposition 4, 1st question: "I think not. See Multiple Erosion Surfaces."

Proposition 5, Question. "Of course not—Anyone can make mistakes in some things and be correct in others."

Report of the Symposium

President Claude H. Birdseye introduced Dr. O. D. von Engeln as organizer of the symposium.

O. D. von Engeln: [The substance of the speaker's opening remarks appears in the beginning paragraphs of this record.]

A number of members are interested in the progress made in translating Walther Penck's work into English. This was begun some years ago as a cooperative project. The attempt was to "convert" his account, *i.e.,* to extract everything significant rather than to do a complete translation. After four or five chapters had been done in this fashion the business bogged down. The remaining contributions were not forthcoming. Then Professor Meyerhoff found a graduate student in German and geology who undertook to do the translation as a thesis. Later Professor Cressey found another student similarly equipped to aid in the work. Meanwhile it appears that one of Dr. Thornthwaite's workers has been engaged on the task. He is now being enlisted in the joint effort. So eventually we may have a complete translation of Walther Penck's work. How to get it published is another question. Any suggestions on this point will be welcomed by any of us who are directly interested in the project.

Chairman Birdseye: The first paper on our program will be presented by Dr. George D. Hubbard of Oberlin College whose subject will be, "Major Objectives of Penck and of Davis in Geomorphic Studies."

Prof. Hubbard then read the following paper:

MAJOR OBJECTIVES OF PENCK AND OF DAVIS IN GEOMORPHIC STUDIES

INTRODUCTION

In Proposition 1 of our Symposium program it is stated that Davis conceived the major objective of geomorphic studies to be the recognition of stages in the systematic evolution of land forms as conditioned by the nature of the cycle, by structure, and by process; and that Penck conceived land forms to be primarily a reflection of the manner (progression) of uplift, and considered the ultimate goal of geomorphic science to be the reading of the epeirogenic and orogenic history of the earth's surface relief, from which reading would come clues of the nature of diastrophic forces. Three questions follow. How may these fundamental differences in objectives, and the differences in approach that they entail, be reconciled? Should reconciliation be attempted? Does acceptance of either invalidate the other?

The formal answers to the questions will be held in abeyance until the differences and similarities between the positions of Penck and Davis have been examined.

A reason why these strong critical students took such different views of the purpose of geomorpholgy may arise from their definitions of this science and of geography. Davis looked upon geomorphology as physiography or physical geography, as the present stage of geology, and as furnishing the topography upon which geography resided. He considered it a part of geography because it is description and explanation of elements of the present landscape. And while he "sought influences and responses," he believed that geomorphology was worth thorough study on its own merits. To him it was the interpreter of past dynamic geology. In this statement he gave it a more significant place than Penck now gives it.

Penck says geomorphology is a border science between geology and geography, and he never has shown much interest in or appreciation of modern geography. Nor does Penck study geomorphology for its own sake. He believes it a far better tool to dig out problems of crustal movements than are structures—tectonics. I am happy myself to see a man tackle geomorphology in this way. I have long believed that every geologic stage had its own physiography as much as has the present, and that close attention to ancient physiographies would illumine many problems of stratigraphy, paleontology, and possibly of ore deposits, as well as of diastrophism as Penck believes.

DIFFERENCES

There emerge several real differences between the work and interpretations of these two scholars; perhaps as many similarities. A look at these differences and similarities before weighing their respective contributions will be in order.

1. The differences in goals in their studies have been mentioned, and to me these seem to grow logically out of their definitions of and interests in geomorphology. If one sees in his science little but a means of interpreting diastrophism, one phase only of ancient geology, of course he omits most of geomorphology that does not contribute thereto. On the other hand, if one sees great values in the science for bases of geography and for interpretation of many phases of geology, as well as values for its own sake he is liable to pursue every branch of it as Davis did.

2. With these differences in goals there must necessarily be some differences in approach and method. Penck seems to rely heavily upon observation, saying that one can see the forms produced and the exogenetic activities, hence the third factor, the unknown in his equation is easily discovered—the crustal movements. Davis was as careful an observer but he was equally interested in theory. His profound use of deduction, induction, and generalization is well illustrated by a comment I heard him use when his terrace studies were in progress. He had searched terraces on three con-

tinents and in many countries, and had made a series of drawings of developing meanders and the terraces that went with them. Drawings had accumulated well up into the hundreds and he was laying aside every 20th one to summarize the series. As he contemplated a table full of drawings spread out before him, he said, "I can think of many more forms than I can find in the field." For his guidance he depended as much on theory as on observation, perhaps more.

3. In the emphasis placed on parts of their science these men differed much. Rarely does Penck seem to inquire systematically into the causes of the forms he studied. Davis could not be satisfied without the explanation. Davis however was not deeply concerned with the deposits made from the waste products removed to make his forms. Penck examines them critically. Both revel in the forms produced.

4. If they differed greatly in goals and approach, they may have differed more in assumptions. Penck seems to assume that time is of little consequence in geomorphology, so much so that some readers say he altogether ignores time as a factor in slope angles. He mentions neither structure nor time in his list of factors responsible for topography. No wonder he could see little in the cycle. His three factors are exogenetic processes, endogenetic processes, and the resulting forms, and, without noting any evidence, he says that all three factors are of equal importance in an equation to be solved for crustal movements. He assumes a balance between exogenetic and endogenetic processes when they are equally effective in an equal length of time without looking to see if materials and structures are the same. For his type case he assumes uplift to be a steady, increasing, upward movement with broadening area followed by a decreasing upward movement to rest. He believes his geomorphologic studies will discover all the types of crustal movements, slow, fast, long, short, intermittent, and continuous, or whatever occurs. He says surface deposits on land do not create new forms but simply smooth out surface relief. Thus the fan, flood plain, levee, island in stream, abandoned lake beds, etc., are not entities, are not creations.

Davis begins his explanation by taking an ideal, not a typical, case of simple uplift rather rapid geologically speaking, followed by long continued erosion, but assumes that his ideal cycle may be complicated by any sort of uplift that Penck assumes to occur and also by warpings and depressions in the various parts of the earth. He assumes that there will be found not only cyclic changes, but diverse types of diastrophism in different parts of the earth and at different periods in the earth's history so that with sufficient search almost any stage and combination of stages may be found in the greatness of time and space. He thinks through many circumstances to see what forms would arise and fits his findings into series and categories.

SIMILARITIES

Penck and Davis in their studies are equally serious, equally honest, and possibly equally, though far from identically trained. Both recognize the several external processes called "exogene" by Penck, and the internal, "endogene" processes. Both recognize agent, process, material, structure, time, and resultant forms in geomorphology but with great differences in emphasis or relative importance attached. Both recognize a great variety of crustal movements but assign different values and frequencies to some types. Probably Penck feels, in placing his emphases, much as Davis once expressed himself when asked why he pushed so hard on a particular process, "By George, I have to—nobody else emphasizes that at all."

ULTIMATE CONTRIBUTIONS

Probably Davis's greatest contribution in geomorphology is in the development of the cycle idea, and in the gathering of evidence in its support, including his discussions of variations in the cycle and its forms, as produced in various climatic conditions, in various rock strengths and structures, and as interrupted by various types of crustal movements.

Penck's contribution will probably lie not in geomorphology at all, but in the light his studies may throw on problems of diastrophism, stratigraphy, and the succession of multiple physiographies.

The questions raised by the organizer of this symposium under Proposition 1 may and probably should be answered in the light of the foregoing paragraphs.

Reconciliation of goals and of approaches to geomorphic analysis should be attempted. Neither student has seen, any more than any of us have, all the possible goals and approaches to such analysis. Each may be right even in most of his details in some part of the earth. Each goal and method of work is a challenge to the other and to any goals which may yet be chosen. Acceptance of one does not invalidate any other; facts discovered, alone, can invalidate. A willingness and ability to see the science in its many possibilities enriches our studies and helps us to come nearer to seeing the science whole. We can differ in our emphases, and even in our interpretation of many forms seen here and there without destroying our science. Only by comparison of goals, methods, approaches, and interpretations are we to understand all the varied surface forms that we see in divers parts of the earth or discover, more or less concealed and mutilated in the records.

Chairman Birdseye: Since Dr. Meyerhoff has not yet arrived, I shall call on Dr. Hellmut de Terra of Carnegie Institution to present the third paper scheduled on our program at this time. After the reading of the paper there will be a short period for questioning these two speakers.

Dr. de Terra read the following paper:

SOME CRITICAL REMARKS CONCERNING W. PENCK'S THEORY OF PIEDMONT BENCHLANDS IN MOBILE MOUNTAIN BELTS

The occurrence of nearly level land surfaces, or remnants of such on our high mountain ranges has puzzled many an observer because such features are generally interpreted as end products of erosion requiring crustal quiescence. Mobility and quiescence are two phases of mountain making; such at least had been the conclusion of certain geographers and geologists who had attempted an analysis of peneplanation phenomena in our young mountain ranges. In some instances, such as in W. M. Davis's report on the ranges of Western Central Asia or in his essay on the physiography of Central France, rolling uplands were considered remnants of a Paleozoic peneplane which was subsequently broken up by strike faulting which in turn led to step-like arrangement of valley-in-valley forms. In other instances, as in the case of our Rocky Mountains or the Sierras of South America it was held that such benchlands resulted from successive uplift beginning with the Eocene Period and continuing into recent times. In both cases students of such land forms agreed that an arrangement of this sort was indicative of intermittent uplift over long periods of time.

This conception was challenged by the younger Penck who had observed that these piedmont benchlands are in many instances independent of structure and that their physiographic character rather suggested their being derived from a sequence of erosion surfaces expressive of continuous and accelerated upheaval of what Davis called broad folds. I may say here that this translation of the German term *Grossfalte* does not convey its true meaning because this type of structure is a ge-anticline with an undulating surface. It would be, in my opinion advisable to speak of undulating ge-anticlines rather than of broad folds, especially as the latter term is used in structural geology to mean a particular type of fold without any relation to surface forms.

Now, W. M. Davis[2] had rejected the physiographic postulates of the younger Penck in regard to the origin of piedmont benchlands chiefly because he could not see how stream erosion could produce valley nicks on a continuously upward moving mountain range. In this many students of landforms will agree but in Penck's theory are other postulates which he held of equal significance when it came to analyze the downstepping type of surface levels. One of these postulates was that accelerated upheaval of a

[2] Piedmont benchlands and Primärrümpfe: *Bull. Geol. Soc. Am.*, vol. 43, pp. 399–440.

mountain range produced a special sequence of rock waste peculiar to basin sediments. The other claimed that in all cases there existed close correspondence between such surface forms and the structural history of the range.

W. Penck had argued that from looking at a sequence of sedimentary columns found on the slopes of a young mountain mass one could detect mountain growth by accelerated upheaval. According to his view the core of a mountain range rises at the beginning so gradually that streams manage to keep graded. The result is a fine grained sediment which reflects the even relief of what he called Primärrumpf. In other words a series of marine deposits will be succeeded by fine-grained freshwater formations. To me this means twisting the geologic facts in order to gratify a physiographic assumption, because in most cases uplift of a land mass is expressed both by tilting (or folding) or by the sudden deposition of a conglomerate indicating torrent action. The Swiss colleagues even go so far as to say that every appearance of a new anticline in the alpine geosyncline was accompanied by instant shedding of coarse rockwaste. As upheaval proceeded, stated the younger Penck, the core of the mountain range broadens, foothills rise and the mountain border grows basinward. Constant acceleration of uplift then caused streams to dissect headward and to quicken the pace of vertical erosion. With uplift becoming ever faster rock debris is deposited in ever increasing quantities and this accumulates in the form of fans growing outward into the basin. The final result, claimed W. Penck, is a sequence of gravel zones which increase in thickness and coarseness as one ascends the formation of the Sierra Ranges in the Argentine where W. Penck did his most intensive work. He claimed that this was true also for many other mountain ranges and that this was a law revealing the correctness of his theory concerning mountain growth by acceleration of upheaval.

In my opinion such an interpretation is as one-sided as it is fallacious. It is one-sided because the observed increase of gravel deposition toward the Pleistocene Period may well be the result of climatic changes. Every land mass which experiences great vertical upheaval passes into another climatic zone where rainfall increases by reason of greater condensation of moisture in the refrigerated upper altitudes. The result may be general increase of stream power and rapid deposition of coarse detritus. A mountain range may have stopped gaining in height a long time and yet its streams continue to transport coarse debris because of a time lag between the establishment of a new climate and the full realization of its effect upon stream work. It is a problem worth closer investigation whether such climatic growth cycles were recorded in such a fashion as to reveal the physiographic determinants of sedimentation between the windward and leeward sides of a growing

mountain range. This is actually the case where such young land masses are uplifted in the established path of moisture-laden winds such as the monsoon of India. Here, the total thickness of the late Tertiary deposits is close to 23,000 feet, while on the northern lee side in Central Asia it is hardly half that much.

Unfortunately the validity of W. Penck's assumption regarding continuous increase in grain size of basin sediments can rarely be noted, for the geological record of basin sediments is incomplete and it is rare that we can actually see the unbroken sequence of sedimentation representing all stages of mountain growth. The example cited by Penck from the Puna de Atacama is an exception rather than the rule. In general the borders of the basin are disturbed by faulting and overthrusting so that most of the sedimentary records of the earlier stages of relief making are lost for observation.

More important in my opinion is the type of sedimentation which is made by slope streams of an uplifted range. Take the Pleistocene gravels for example. They generally fill valleys which were eroded at the close of the Tertiary. Then the range was in a state of uplift and streams were not graded as the Ice Age overtook the normal stream cycle of erosion. Then followed extensive alluviation, interrupted at times by erosion. Although it is established that Pleistocene alluviation was aided by a glacial climate it is nevertheless obvious that such alluviation was due to increase of stream power. There must also have been a stable baselevel of erosion because in most instances such gravel terraces can be followed clear through the transverse valleys into the neighboring basin. Hence there must have been temporary stability of baselevel. In fact there are examples known from the Alps, the Himalayas, and especially the Dutch East Indies. There mountain ranges are in their infancy and illustrate the process of intermittant uplift and tranquility. In some cases such levels as were derived from prolonged alluviation are tilted and succeeded by erosion levels which show no effects of structure. Cornelius has described terraces of first interglacial age in the eastern Alps which were rotated at the foot of an anticline that underwent uplift during the following interglacial.

Wherever Pleistocene mountain making was recorded we notice that it proceeded by intermittent jerks or paroxysms. The same type of mountain growth prevailed during the Tertiary Period, for how else could we interpret the sudden appearance of coarse conglomerates at the base of a new sedimentary sequence?

All this seems to be a truism but when challenged as W. Penck did, it is well to recall certain results of research which were gained more recently and which seem to argue strongly against the conception of continuous

accelerated upheaval as held by the younger Penck.

The next question is what conclusion we can draw from this knowledge in regard to the origin of piedmont benchlands. Are such valley-in-valley forms as observed and described by Penck and by others necessarily expressive of structural evolution? In the short time at my disposal I cannot give a full analysis of this question but will restrict myself to one of the features of piedmont benchlands, namely the remnants of peneplane levels or even upland surfaces on the crest of young mountain ranges. It is true what W. Penck said about their presence in all parts of the world. They are universally present on ranges which experienced their first upheaval at the close of the Mesozoic through the Laramide orogeny. In the Rocky Mountains such upland surface is considered a remnant of an old Eocene relief. In the Alps the highest plateau remnants and bench levels are held to be Oligocene and Miocene. For the Tibetan Plateau, which is probably the most striking example of all, Machatschek[3] has recently claimed Eocene age, pointing out that this comes nearest to what W. Penck called a Primärrumpf. I happen to know that upland from my travels and believe that it constitutes neither of the things that have been ascribed to it. In its formation are involved factors other than stream erosion and it is to these that I want to call special attention because it is possible they were equally instrumental on other mountain ranges at one time or another.[4]

Like most of these flat surfaces, the Tibetan Plateau is not a peneplane but a surface in a late mature or old stage of erosion. So at least it would seem to the casual observer or to the student who glances over a map. There are glaciated ranges which surmount the valley and basin floors by 5,000 to 8,000 feet and these are separated by wide basin-like valleys often dotted with salt lakes and fringed by the lacustrine deposits of very extensive freshwater lakes, now extinct. The curious and singular feature about this landscape is that there are no perennial streams. Nor is there a normal drainage. Streams issue here either from a salt lake or from a snow bed or piedmont glacier. Most of them never reach the edge of the plateau but vanish gradually in the alluvial soils. In fact many of these broad valleys have no streams at all and yet their flats are often two to three miles wide. Despite this lack of stream action slopes are in recession and valley flats are forming as if some unseen process was continuing the work which streams once performed when drainage was normal. There can be little doubt as to the stream origin of the valleys because of the normal arrangement of the drainage lines and also because of the stream deposits at the bottom of some of

[3] *Das Relief der Erde*, Berlin 1938, vol. I, p. 312.

[4] A more exhaustive treatment of the origin of alpine level surfaces, backed up with a great number of illustrations, will be published in the near future.

these depressions.

The process molding this landscape and causing it to lose constantly in relief is chiefly one of physical weathering under subarctic, arid conditions. With annual precipitation of eight to twelve inches per annum and frost conditions prevailing most of the year rocks are broken up rapidly especially as there is practically no vegetation. The result is formation of slip and structure soils which cover the entire landscape. Chief factors in their formation are nivation, frost weathering, insolation, and an incomplete drainage. The action of snow on barren rock is particularly effective during the early summer when winter snow fields are melting and fresh snow falls in daily flurries. The fresh snow, however, melts rapidly and moisture evaporates very fast due to intense radiation. There are perpetual snowfields in certain sheltered valleys and these usually cause a local deepening of the depressions. It is here that one encounters larger quantities of loam which is apparently derived from decomposition of feldspar minerals. Once exposed to intermittent soaking with snow water, the rock begins to crack into large, angular blocks. These in turn disintegrate into smaller fragments when frost action and insolation have sufficient time to function. As in the case of nivation these processes work most effectively when there is constant freezing and thawing going on as is actually the case for at least four months of the year. Then diurnal temperature differences often amount to over 100° F. At altitudes of 17,000 to 18,000 feet, insolation is twice as effective as at 10,000 feet, as I had occasion to experience on my own skin. Under such extreme conditions rock surfaces are literally blistering away, a process which is often accompanied by violent detonations resulting from sudden cracking of boulders. The resulting soils consist of angular debris with a 10 per cent loam component. They undergo constant mass movement, slipping downhill, either in form of miniature avalanches or sliding lines of boulders. This mass movement or solifluxion is arrested in the valley bottoms. Here the particles are reassorted to form structure soils with polygonal patterns. But as the valleys fill up their slopes recede, for with the gradual filling and heightening of the valley floor goes what I would call a temporary level of lateral frost action. For snowfields and groundwater ice undercut the slope, causing debris to slip further downhill. The penultimate result of such slope recession is a general lowering of interstream divides and a progressive widening of valley floors. In such wide, alluvial flats headwater streams of the slope drainage, advancing toward the Tibetan Highland, lost their cutting power so that their advance toward the upland is arrested by solifluxion and freezing. This actually enables the waning of the landscape to proceed while the slopes of the mountain range are undergoing dissection. Such peneplanation by weathering may

well proceed simultaneously with a young dissection of the slope region.

Weathering processes such as these have been observed in the Rocky Mountains and in the Alps. They may be less effective at lower altitudes but it should be recalled that the central portions of young mountain ranges have passed through some sixty million years of history. Within this time-span these uplands must have been repeatedly subjected to conditions of waning such as I have described. It is true that in most cases normal stream erosion had already proceeded toward a late mature or old relief; but when such surfaces were subjected to high alpine weathering, the waning must have developed at a much more rapid pace. Our example illustrates that glaciation is not a prerequisite to such a waning development by solifluxion though obviously glacial conditions would quicken the process considerably. In my opinion it is very probable that such a leveling of upland forms had already taken place during the late Tertiary when many younger ranges had attained maximum height.

In regard to W. Penck's theory, this means that the "central highland" does not necessarily have to represent the geologically oldest land surface. It should rather be regarded as the composite product of old stream erosion and more recent weathering. In fact, many of these surfaces need not be older than late Tertiary. Therefore to me it would seem unlikely that piedmont benchlands express in all instances a lawful evolution of mountain growth even though intermittent uplift took place. It is essential to bring all climatological and geological data to bear on the analysis of such complex land forms as piedmont benchlands.

Chairman Birdseye: We have a very short time now for discussion of these two papers, Prof. Hubbard's and Dr. de Terra's.

President W. W. Atwood, Clark University: Let me ask Dr. de Terra, did I understand him to say this was true of the whole surface of the plateau?

Dr. de Terra: It was originally in a late mature stage.

What I found was that the valley floors were underlain by structure soils and these in turn by fine grained river alluvium. No fossils were found in this formation so its age remains unknown but the character of the sediments does not admit any other interpretation but deposition in slow flowing streams. This sort of deposit goes with a mature relief such as must have existed at the close of the Tertiary. Already then uplift had carried these highlands into great altitudes causing a drier and colder type of climate. From that time on slope recession was greatly augmented by solifluxion causing a general waning of the mature relief and ultimate formation of rugged interstream divides and very wide basin floors.

President Atwood: How far out from the present mountain pass did you find the angular material? Did it carry out the solifluxion idea across the

whole plateau?

Dr. de Terra: Yes, sir. Solifluxion soils could be seen to cover both slopes and basin floors over the entire length of our route.

Prof. V. C. Finch, University of Wisconsin: I believe that the mechanical processes of weathering are highly overrated, the chemical weathering underrated. The speaker seems to indicate that he thinks temperature changes highly important elements. In reality I should think that would depend entirely on the altitude of the range. It is quite possible that during maximum uplift, in the Pleistocene (usually associated with maximum uplift) they applied, nevertheless these conditions are optimal, and at that time the physical breakup certainly proceeded much faster than later on or at lower levels, lower altitudes. I think it depends entirely on local climatic conditions, insolation, nivation and so forth.

Chairman Birdseye: I think we had better leave further discussion until the end of the period. Prof. Meyerhoff of Smith College is here now, and I am going to ask him to present his subject.

Prof. Meyerhoff then spoke on the topic

MIGRATION OF EROSIONAL SURFACES

As I have listened to the carefully prepared papers of my two predecessors on the program, I have become increasingly embarrassed. I almost wish the train that brought me had been late in order that I might have had the best of excuses for failing to match an impromptu presentation against their excellent contributions to the moot subject of Penck's geomorphology.

I have tried several times to get down, in written form, the things I have come to believe true, partly true, or at least possible from data and experiment, but chiefly from observation of geomorphic phenomena; and I finally decided to leave at home the partly prepared manuscript which I wrote for this symposium. If, in presenting my views, I sound a little confused, you may assume that I am very much confused; and I believe that the status of geomorphic thinking, particularly geomorphic thinking on the evolution of erosional surfaces, is as badly confused as I.

As American geomorphologists, we all have upon us the mark of the American school of thought, developed so convincingly by William Morris Davis. If we adhere to the precepts of this school, we are promptly handicapped by a tectonic premise which, I fear, does not stand too close scrutiny. A tectonic premise that assumes very long still-stands of the land and the sea would permit tremendous areas to become effectively peneplaned. But have we any basis whatever for believing that any part of the earth has remained in a constant position with respect to the sea; long enough, for example, for the development of such a vast surface as many physiographers

conceive the Laurentian Peneplain to be? Possibly I am utilizing a rather sharply defined concept of a peneplane, but all of us are too prone to take over the term and use it as our own particular word. We feel that with a little explanation we can employ it as we wish, but so does the next man, and the next. The formulation of our own classroom definitions is partly responsible for our geomorphic troubles. Even so, when we analyze the concept of a peneplane as it is most generally held, I am quite sure that many of us believe that we may have a peneplane millions of square miles in extent—in the case of the Laurentian Upland, one and a half million square miles, at least. And many geologists are convinced that it is a simple surface representing degradation with respect to a still-standing sea, which tenaciously held the same position for a tremendously long period of time. A few geomorphologists have appreciated the fact that stratigraphy records no such still-standing sea at any time or in any place during geologic history. To our embarrassment we must accept the dicta of another branch of geology, and admit that the sea goes up and down a great deal more rapidly, a great deal more frequently than would permit the development of an erosional surface like the Laurentian Upland as a single, simple geomorphic form.

In the face of stubborn facts, some of us have tried to dodge the issue. One of the dodges used in the past has recently been revived: I refer to the dodge of pushing back into the remote past in an effort to explain such erosional forms as the Laurentian Upland. It has been argued that the Laurentian surface is a Lipalian peneplane, formed in pre-Upper Cambrian time and re-exposed by stripping. When we come to analyze this suggestion, we can go into Wisconsin and northern Michigan and discover that here the pre-Cambrian surface was rugged, for Upper Cambrian rocks rest upon a surface that has over one thousand feet of relief. R. M. Dickey and F. T. Thwaites demonstrated the high pre-Cambrian relief in field trips sponsored by the American Association for the Advancement of Science in June, 1939; and I have studied evidences of moderate relief along the margins of the Adirondacks, too, where varying thicknesses of the Potsdam sandstone and the overlap of Ordovician strata, indicate a hilly topography, at least, with hills on the pre-Cambrian floor rising over 700 feet above the valleys. In fact, that much relief is still left, in spite of subsequent erosion. Studies by Dake and Bridge in the Ozarks have likewise revealed early Cambrian hills rising more than 700 feet through the enveloping Cambrian formations; and in the Black Hills of South Dakota field studies have brought to light Cambrian sea cliffs, one of them 150 feet high. So pre-Cambrian peneplanes, subsequently stripped, appear to be little more than pious hopes which are unutilizable as geomorphic forms; in fact, it looks

as if the Lipalian peneplanes which are featured in our textbooks are mere figments of active imaginations. We cannot accept a dodge of that kind.

Recently, a few workers have concluded that elevated peneplanes are not quite so inviolable as has been thought. N. M. Fenneman and John L. Rich have argued for uninterrupted lowering of raised erosional surfaces, but George H. Ashley is the great radical on this score. He has recently expressed the opinion that the Appalachian surfaces have not only been greatly dissected but have undergone general lowering as a result of post-Miocene erosion. Fenneman argues that upland levels which are called peneplanes are not the original planed surfaces but something lower which has been formed by slow reduction, acting regionally on any country lying above the grade of its major streams.

We may profitably re-examine Fenneman's hypothesis, but in doing so it is necessary to consider its application to two distinct types of elevated country. First is a region which is thoroughly dissected by a fine network of streams, such as we find in parts of the Appalachian Plateau of West Virginia. Here, as dissection approaches maturity, knife-edge interstream divides develop, and there will come a time when the level skyline of the original eroded upland will be precariously preserved only in the crests of the slender, steep-sided ridges that drop sharply to the deeply incised streams. Obviously, when dissection proceeds beyond this point, lowering of the knife-edged ridges and of the skyline must ensue; and under special and rather ideal circumstances, rarely realized in nature, it is possible to imagine the divides being reduced at an even pace, each preserving the same elevation, and all giving a gross impression of a level upland surface of erosion which never existed. At the meetings of the American Association for the Advancement of Science at Columbus, John L. Rich illustrated his paper on "Identification and interpretation of erosion surfaces in the Allegheny Plateau" with air views, some of which caught portions of the West Virginia plateau in that brief moment of geologic time in which nearly all vestiges of the initial flat surface are gone, but in which the knife-edged divides still retain the approximate elevation of the original plateau. No one can see this part of the Allegheny Plateau or Rich's pictures, some of which have been reproduced in a recent issue of the *Geographical Review,* without believing that the entire skyline will soon suffer reduction, and that any attempt to correlate the lowered ridge crests, however convincingly they may integrate into a pseudo-peneplane, would be doomed to failure, for even the skyline profile of the initial upland will have been destroyed.

On the other hand, we must seriously question the applicability of Fenneman's hypothesis to those broad, flat upland remnants in which there has

been no stream incision. I have in mind not so much plateau elements in which resistance may be attributed to a stubborn cap-rock but country which is underlaid by shales, or preferably by a variety of rock-types, such as sandstone, limestone, and shale. One of the most instructive examples is the country comprising the South Dakota Bad Lands. At Scenic in the west, or at Cedar Pass in the east, the observer may suspect that "the wall" which marks the break from the Missouri Plateau to the White River lowland, is held up by resistant strata. But the plateau surface may be traced from either extremity of the Bad Lands belt toward the middle at Big Foot Pass, and here there is nothing but the soft clay of the Oreodon beds from the top to the bottom of "the wall." Indeed, close scrutiny shows that lithologic controls are negligible throughout the Bad Lands country, and that the Missouri Plateau is placidly evolving its geomorphic destiny in a cycle that is not affected by the entrenchment of White River until the headward-receding wall suddenly and unceremoniously interrupts. Regardless of the underlying rock-types, the plateau is a gently graded surface, in equilibrium with the scant rainfall, the sparse vegetation, and the abundant clastic debris; and this equilibrium is maintained over the entire surface, commonly within a foot of the precipitous break or "wall" into the White River valley.

The character of "the wall" or erosional scarp separating the White River from the Missouri Plateau surface deserves further study and analysis. It fully merits the local name, "wall," which has been given it, for its vertical sides make an ascent or a descent all but impossible, except at the widely spaced "passes." In detail, it exhibits a highly crenulate gully-pattern, but the horizontal distances between gully-heads and spur-ends is not large—only rarely exceeding a mile and most commonly being less than 500 yards. Unreduced outliers, severed from the plateau surface, are common in the Scenic section, because in this locality there are several sandstone horizons which provide protective cap-rocks for spurs and remnants. The significant fact is that "the wall" is a retreating erosional escarpment, moving steadily away from White River, notwithstanding the fact that recession is not facilitated by a single large tributary stream. The temptation to attribute recession to gullying is strong, but the fact is that the spurs retreat with almost equal speed, and the secret of recession is not to be found in "the wall" but in the character of the slopes at its base.

The White River lowland, broadly considered, is a graded surface, and any profile drawn normal to the stream and up to "the wall" is virtually an ideal profile of equilibrium. The angle may be slightly in excess of five degrees at the base of the escarpment, but it becomes progressively lower toward the river. Although the slope is channeled, its gradient is com-

pletely dominated by White River, and it is obvious that rainfall can course over every part of it as sheet-wash. For this reason it seems appropriate to call graded slopes of this kind *wash-slopes,* a term which I believe is equivalent to Penck's Haldenhänge. They are mature slopes, at grade with respect to the forces which are fashioning them—namely, the controling mainstream, White River; the volume of water which courses over the surface during the spasmodic rains; and the load of clastic debris which must be moved down it from "the wall" to the stream. That the load is a vital element in determining the angle of a given wash-slope is demonstrated not only by the minor variations which occur in the White River lowland, but by greater variations in the desert valleys of Nevada, where coarse gravel and pedimented surfaces may attain profiles of equilibrium as steep as 12 or 15 degrees. In any given situation, the wash-slope is the composite product of sheet volume and load, and the angle required by these local factors will be developed alike on degradational forms (pediments, valley slopes, etc.) and on aggradational forms (chiefly fans). In humid regions, with abundant precipitation and deep chemical weathering, the ratio of water volume to mechanical load is high, and the wash slopes attain low profiles; in arid regions, the ratio of water volume to load is low, and the profile angles are high.

The recession of the erosional escarpment at the head of the wash-slope profile is almost entirely dependent on the rate at which gradational processes extend the profile of equilibrium on the wash-slopes. The processes involved are analogous to those which have been carefully studied and described along shorelines, where recession of the wave-cut cliff is conditional upon the capacity of the undertow to grade the marine profile and to move debris from the shoreline into deep water, beyond the limits of gradation. I recognize the claim that gullying will operate on any steep slope, and that gravity, abetted by water, will tear it down by slumping, soil creep, and other processes which have been so effectively called to our attention in recent writings of Sharpe. Because these scarps are prey to the varied effects of gravity, they may appropriately be called *gravity-slopes,* and the forms which they assume will depend entirely upon the coherence of the materials of which they are composed, and the local processes which effectively operate upon them. They are ungraded, immature; and they recede only as gradation of the subjacent wash-slopes enables sheetwash to carry off the clastic debris derived from them. The two types of slopes rarely, if ever, merge, for gravity-slopes are seldom gentler than 22 degrees and they commonly lie at the angle of repose; wash-slopes seldom exceed fifteen degrees, though there may be a shadowy and imperfectly known transition between the two in those depositional forms known as talus cones

and fanglomerates. Although I am not entirely sure that these two terms are precise translations of Penck's terms Haldenhänge and Bösche, I am reasonably sure that wash-slopes and gravity-slopes are respectively equivalents of the two concepts which feature so extensively in Penck's work.

Now the main thesis which I would like to present to you this afternoon is this: That we need to make careful examination of the validity of the wash-slope, gravity-slope concept of fluvially developed landforms. I have not put it forth as something definitely proved—the last word, to be accepted by all present without further question. On the contrary, it is an hypothesis which demands, not classroom skepticism, but meticulous testing in the field. We must find out whether every unglaciated landscape may not be broken down into these two types of slopes—the wash-slope and the gravity-slope. And if this proves to be the case, are we not compelled to reinterpret our fluvial landforms along the following lines.

First, with regard to tectonics: the evidence of stratigraphic history effectively precludes the premise on which the idea of regional peneplanation is based, and it also rules out the equally extreme premise on which many of Penck's geomorphic theses are based—namely, that the earth is in constant differential motion, either at an accelerating or decelerating tempo. Both these diametrically opposed premises are contradicted by the facts of stratigraphy, and there is only one course of action for us to pursue —that of throwing them both out and reinterpreting our land forms in harmony with the findings of more firmly rooted branches of geology. Accordingly, we must accept the concept of a restive earth, in differential but intermittent motion. Generally the positive continental elements are elevated, the negative oceanic elements are depressed. Reversals occur in this order of things, somewhat commonly in the low continental interiors, but more rarely in the mountain borders which characterize most of our large land masses. In the latter each uplift is followed by stream entrenchment and the inauguration of a new fluvial cycle. The latter may proceed to maturity or possibly old age; but an interruption is bound to come and another partial cycle will ensue. At each successive level, lateral planation is begun by the streams after they have attained grade. In picturing what happens when the next uplift takes place, we are seriously handicapped by two unfounded assumptions; first, that stream entrenchment occurs at once, from stream mouth to stream source, in all the river systems; and second, that after entrenchment occurs, the stranded valley floors become ornamental strath terraces in which evolution comes abruptly to a stop.

This is not the time or place to expose the fallacy of the first assumption, but suffice it to say that entrenchment proceeds in a leisurely fashion from stream mouth to stream source, with each successive set of cyclical forms

migrating progressively upstream. Indeed, it is not impossible that upstream erosional forms which have developed in Quaternary time were inaugurated by uplifts which occurred near the stream mouths at a much earlier geologic date. This is especially true in such long drainage systems as those which head in the Rocky Mountains, and not only must the dating of landforms be carefully reviewed and revised, but the dating of diastrophic events on the basis of geomorphic phenomena should be held in abeyance until geomorphologists appreciate the fact that earth movement of an epeirogenic character, and physiographic adjustment to it are not necessarily simultaneous or even immediately successive events.

The second assumption, it is hoped, has been shown to be unfounded by the material presented in this paper. However rapidly lateral planation by streams may broaden valley floors, the process of valley floor gradation is also carried on by sheetwash in the development of graded wash-slopes. When stream work is halted by entrenchment, sheetwash continues on the strath terraces which are left, and the process is accompanied by the steady, if slow, recession of the erosional scarps or gravity-slopes which mark the limits of the strath terraces. In a recent paper, published in the *Bulletin of the Geological Society of America,* John L. Rich has challenged the interpretation of multiple erosional surfaces in New England and in the Rocky Mountains, primarily because the scarps and surfaces are so fresh that he cannot conceive of their preservation from comparatively remote geologic time to the present. Rich is correct in questioning the ancient geologic dates assigned to erosional land forms, but he has completely overlooked the uninterrupted development which these surfaces undergo through wash-slope evolution and gravity-slope retreat. And much as one may quarrel with certain of George H. Ashley's recent conclusions, there must ultimately be agreement with his main thesis—that very little which we see in any landscape dates back much farther than late or middle Tertiary time.

To geologists who have long dealt with, and taught, the persistent and unceasing activity of such dynamic agents as rivers and rain-wash, there should be nothing radical in the interpretation of land forms as evolving and migrating features of every landscape. Indeed, it is difficult to understand how the prevailing viewpoint of static land forms, whose development is mysteriously arrested by uplift, can meet with such ready acceptance, while the dynamic interpretation offered by Penck and elaborated here, with any apologies which may be due Dr. Penck, is looked upon with skepticism, and in some quarters with downright hostility. Although we must discard Penck's diastrophic premise, we must also abandon the diastrophic premise with which the Davis school has unnecessarily burdened itself. Once these impediments to straight geomorphic thinking are removed, it seems reason-

able to believe that the fluvial cycle will find integration in the concept of gradation, and that the evolution and migration of wash-slopes and the orderly recession of gravity-slopes will occupy an important place in the interpretation of land forms. Pediments will take their proper place as normal wash-slope features, and the highly exaggerated contrasts in arid and humid environments, which are being stressed in current literature, will be appreciated as natural but systematic responses to climatic factors. Geomorphologists may then have less to argue about, but at least they can call their chosen field of study a science.

Chairman Birdseye: The next speaker on the program will be Professor Bryan of Harvard University. *Professor Bryan.*

THE RETREAT OF SLOPES

THE OPPOSING DOCTRINES

The impact of the radical doctrines of Walther Penck (1924) on the American school of geomorphology has all the effect of a cold shower on a complaisant reveler. Slightly bemused by long, though mild intoxication on the limpid prose of Davis's remarkable essays, he wakes with a gasp to realize that in considering the important question of slope he has always substituted words for knowledge, phrases for critical observation. He has been lulled into complaisance by the sonorous expressions: "retreat of slopes," "gradual reduction to a gently undulating plain," "the smooth and gentle slopes of old age." Suddenly faced with a challenge to his long-practiced incantations, he finds himself unprepared to defend these ritualistic formulae.

The viewpoint of the present writer is that the most important part of Penck's challenge lies in its revelation of our ignorance of the nature and character of slopes. However, Penck himself held that he merely challenges Davis's endogenetic assumptions, *i.e.*, because Davis ignored erosion during uplift, the theory of the Cycle of Erosion breaks down. In this rebuff to Davis, Penck did an injustice. As early as 1892 Davis specifically stated that uplift was complete before much erosion had been accomplished. Yet he was the first to describe the initial erosion and the preliminary forms produced in the early part of uplift and during progressive uplift as set forth in his paper on the Basin Ranges (1903, pp. 730, 740–755).

The only basis for such a charge lies in Davis's method of setting forth the doctrine of the cycle. He consciously and usually with an explanatory paragraph or sentence, ignored erosion during uplift and began his exposition of the processes of land sculpture on a mass already uplifted. In the use of this forensic and pedagogical device, Davis may have deceived a few

of his readers, but certainly never himself. It is, of course, true enough that Davis shared the opinion common among American geologists that uplift is rapid whereas erosion is slow. Also Americans hold that in general great uplifts occur in steps by means of intervals of rapid uplift interspersed with intervals of stability. Erosion during the intervals of stillstand may develop land forms which remain as evidence of the pause in uplift. Such intervals are usually regarded as "partial cycles."

This concept of relatively rapid uplift is supported by much evidence in many well-studied areas, and has been vigorously defended by Davis (1932, pp. 405–408). But all the evidence should be reconsidered in the light of Penck's challenge. Do straight slopes, or *Gleichförmige Entwicklung,* furnish valid evidence of slow uplift *pari passu* with downward degradation? Are convex slopes or *Aufsteigende Entwicklung,* critical determinants of rapid uplift exceeding the ability of the processes of degradation to lower the slopes? The writer holds that there is sufficient evidence to show that Penck's contention as to these two types of slope development, however theoretically possible, is not supported by the field facts. To cite these facts and to discuss the problems involved would require many pages.[5] This essay is confined, therefore, to a discussion of the third type of slope development in which Penck, himself, postulated that uplift is complete and that base level is stable or at most rising very gradually. With such a condition Penck associated concave slopes or *Absteigende Entwicklung.* As topographical slopes concave to the sky are the common feature of nearly the whole of the land surface of the world, it follows that stable base level is dominant in the development of our modern topography and presumably that this condition has existed throughout the period of time involved in that development. The interruptions, the sudden and relatively rapid uplifts of the currently accepted concept, merely punctuate the phases of stability and produce the valley-in-valley profiles concave in detail and also usually concave as a whole.

Penck divides the concave slopes of Absteigende Entwicklung in two parts. The steep slope or *Steilwand* and the gentle slope or Haldenhang which is succeeded toward local base level by still gentler slopes, the Subhaldenhänge. Thus the concave slope is conceived as a union of slopes which taken together merge into a smooth curve somewhat resembling a parabola. In the area between two parallel streams (the local base level) the steep slopes from either side retreat until the original highland is consumed and, thereafter the divide is lowered and the relief of the area re-

[5] The attempt by Sauer (1929) to apply the Penckische system to the Peninsular Range of California has met with severe criticism, Bryan and Wickson (1931), Miller, W. J., (1935).

duced. The steep slopes continue to retreat until the upper part of the ridge is consumed and the gentle slopes or Haldenhänge retreat and disappear to be replaced by successively lower Subhaldenhänge until the *Endrumpf* is reached.

If, for the moment, we disregard Penck's assumption that this retreat of slopes occurs by the downward migration of thin slices actuated by gravity and made mobile by weathering, the whole process is suspiciously like the progress of the Cycle of Erosion. At the beginning the slopes are steep—youth; the divides are narrowed and become knife-edge ridges—maturity; the ridges are lowered and their slopes become gentler—old age; finally they become very gentle, so flat that the process of reduction is almost infinitely slow—the peneplain or the penultimate stage before base level is reached. Thus it appears that the Endrumpf of Penck, in spite of his categorical denial that it is not the peneplain or *Fastebene* of Davis, can be nothing else as Davis was quick to claim (Davis, 1932).

Our concern is not with the supposed distinctions between Endrumpf and Fastebene or between Primärrumpf and uplifted peneplains but with the form of slopes during the progress of the cycle. In brief Penck holds that steep slopes retreat without loss in their inclination, and that steepness disappears only because the land above the grade of the Haldenhänge has been consumed. Gentle slopes replace steep slopes at the time when the Haldenhänge meet on the divides. On the other hand, Davis holds that slopes flatten during the progress of the Cycle of Erosion, from youth through maturity to old age. It is true that Davis in the latter years of his life recognized the advances made in the study of arid topography and in a curiously worded group of sentences, admitted that his generalization would not hold for arid regions (1932, p. 410). In his paper on "Rock floors in arid and in humid climates" (1930, pp. 7 *et al.*), he repeatedly contrasts the steep slopes of residuals in arid regions with the "gently sloping soil- and plant-covered residual mounts and mounds that curve down gradually to soil and plant-covered peneplains in humid climates."

It is questionable whether Davis's position is correct for humid climates, and in this paper a review of the question is given which seems to support Penck's contention that with stable base level the Steilwand retreats without loss in steepness not only in arid but also in tropical and humid climates. It does not follow that any other of Penck's theories are thereby supported or even that his analysis of Absteigende Entwicklung is adequate or complete. It is only a closer geometrical approximation to the actual than that of Davis.

DAVIS'S GENERALIZATIONS REGARDING SLOPES

Long-continued reading of Davis's papers yields very little material on

slopes. Phrases and catchwords may be found here and there, but there is no study of the processes that produce slopes nor even a recognition of the work of others.

In 1898 there appeared an abstract with the title, "The Grading of Mountain Slopes," but no essay followed. Only general statements are made as to the gradual flattening of slopes during the progress of the cycle. As a result of his reading of Penck (1932, pp. 25–27, 136–138), there is a frank comparison of humid with arid erosion and an adoption of the analysis of arid forms to explain the humid. "Valley floors" and "foot slopes" are the equivalent of the pediment and are distinguished from "valley walls" which are like the hill slopes of arid regions but are of more gentle declivity, and more rounded at the top.

THE TEXT-BOOK DIAGRAMS

The general lack of clarity in the concepts held by American and other geomorphologists who, in general, follow Davis's lead, can best be understood by a consideration of the diagrams published in textbooks. The words used by the textbooks in the description of the cycle are more difficult to interpret, but the diagrams show clearly to what extent the authors have thought through the problem. It must be admitted that many of these authors seem to have realized that the doctrine of the Cycle of Erosion was inadequate or at least incomplete. It is obvious that they were cautious in their wording and dubious of their diagrams.[6]

The famous three-volume *Textbook of Geology* by Chamberlain and Salisbury, published in 1904, the several editions of this book in shorter form, and Salisbury's *Physiography* published in 1906 all have the same diagram. The diagram shows successive stages in the development of a valley in a plateau. Edges of the youthful canyon (first stage in the diagram) are rounded, and this convexity is retained until stage 3 is reached. Without explanation in the diagram or in the text, the slopes in all later stages are shown as concave. The caption of the figure and the text avoid any statement as to which profile in the figure represents maturity. Further, the authors avoid the term peneplain and use base level and base-leveled surface to describe a land surface, and thus make this term equivalent to the peneplain of Davis.

Tarr and Martin in their *College Physiography* published in 1914 also use a diagram of a valley. This diagram does not show the condition of the divides between streams at any of the stages which are indicated. It

[6] Comparison of the textbook diagrams has been a feature of the writer's lectures for many years. A former student, Dr. George S. Cressey, has recently made the same comparison as here given in a widely circulated mimeographed manuscript.

merely implies that slopes, slightly irregular but generally straight, have lower and lower inclinations as the cycle progresses.

Hobbs's *Earth Features and Their Meaning* is a book characterized by many simplified drawings to illustrate typical land forms. The diagrams on page 172 are unusually definite in the delineations of the cross profiles of valleys. Infancy is shown as a narrow valley with very steep walls slightly rounded at the top; youth is similar with more flaring slopes; adolescence has still flatter slopes and an incipient floodplain. The valley walls of maturity are a doubly inflected curve, the ogee curve or *cyma reversa* of architecture which Hobbs refers to as "Hogarth's line of beauty." There is a marked floodplain. In old age, the valley walls have disappeared and the floodplain predominates. Although the text comments on these profiles, there is no explanation of the detailed processes by which these sloping valley walls are produced, nor any argument as to what changes in process occur by which changes in the form of the slopes should follow during the progress of the cycle.

Other textbooks are equally vague or completely avoid specific commitments in the form of diagrams and describe the slopes at the several stages of the cycle in conventional phrases derived from Davis's essays.

Davis (1932, p. 408, fig. 4) treats of this problem specifically and contrasts his viewpoint with that of Penck. In his drawing, here reproduced in Fig. 1, the set of profiles on the right represents his view and that on the

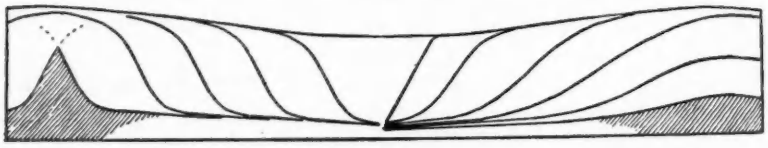

FIG. 1.—Widening of valleys and change in slope profile during the cycle. On the left, according to W. Penck (Absteigende Entwicklung). On the right, youth to old age according to W. M. Davis. [From Davis (1932).]

left that of Penck's Absteigende Entwicklung. Disregarding for the moment the exaggeration of the progressive downward cutting of the floodplain in the right hand profiles, Davis contends that the straight slopes of youth become doubly inflected almost immediately because the angle between the slope and the original plateau surface is attacked by weathering from two sides and is thus rounded off. This rounding becomes progressively greater throughout the cycle producing the moundlike forms of the later stages. All experience, however, leads to the conclusion that erosion on flat slopes is negligible and that the only effective action is on the in-

clined slope. Rounding occurs only if the rock is fragile. If the water table is deep, the corner may be sharp as held by Wentworth (1928, p. 397) in his study of escarpments in Hawaii.

GENERAL STUDIES

Published studies of slopes are relatively few and of these the best is also one of the earliest. In a handsome quarto-volume with an atlas, G. de la Noë and Emm. de Margerie discussed *Les Formes du Terrain* in 1888. Their discussion is specific, and slopes are described as generally concave to the sky, as due primarily to the work of running water, as separable into slopes produced by rivers and streams, and those produced by rain wash on hillsides. The production of the slopes of hillsides is described as a gradation to the base of the slope, usually a stream, which becomes the control or "directrix" from which the curve of the slope is generated. It is recognized that the outcrops of rocks of differing resistance produce slopes of differing declivity; that where two or more unlike rocks crop out, compound slopes are formed and that by suitable combinations of these rocks the concavity may be accentuated or the slope may be broken into two or more concave slopes or may even become a generally convex slope. It is also recognized that certain rocks, such as chalk, produce slopes which are inherently convex and thus form exceptions to the general rule of concavity in slopes. Further, it is recognized that creep, landsliding, the overturn of trees, minor gullying and other processes occur and produce irregularities in slopes. These irregularities tend to be ironed out by the effect of rainwash. Further these authors recognize the influence of climate on slope and hold that the same rock may maintain a different slope under differing climates, so that considering this factor, there is no one slope unique to any one rock.

This memoir provides a firm starting point for the further elaboration of the problem of slope and compared to it few of the later studies are of great importance. However, it has been largely overlooked and little quoted in the literature.

The rounding of summits so characteristic of many landscapes has attracted attention from time to time. Thus Gilbert (1909) noted that divides are rounded and attributed the form to the dominance of creep. Davis (1898) had previously commented in a note. Lawson (1933) in a recent essay considers the rounded summits of hills in humid regions and assumes that during the progress of the cycle of erosion the "round" at the top of the hill flattens and has an increased radius of curvature. With this artificial and unproved assumption as a basis, he discusses the rate of reduction of hills. An elaboration of the subject of hill-top rounding, obviously

closely related to slope erosion, would unduly lengthen this paper, and it is postponed to a later time. It should be noted that Lawson's assumption is in line with Davis's frequently expressed doctrine. It implies a contrast in form and consequently in process as a result of morphological development in humid and in arid climates.

In a discussion of the rounding of hill tops, J. E. Marr (1901) classifies slopes and asserts that straight slopes are characteristic of deserts whether of polar or warm types, and that concave slopes are the result of running water as a dominant process, and thus are expectable features of humid climates. Rounded tops are related to the peat cover on hill tops which occurs characteristically in the west of England. The peat gives rise to a special type of hill, the "moel." The climate of England is temperate, but also marine. Surface peats are fairly common in marine climates, but are rarities in the continental type of temperate climates. Thus Marr's discussion is not generally applicable to the hill-tops and the hill-slopes of temperate climates, but his recognition of the relative steepness of desert slopes was an acute observation in advance of his time.

SLOPES IN ARID REGIONS

Passing over casual statements and incidental comments made by many students of arid regions, the first general analysis of the slopes of arid regions is that by Lawson (1915, p. 29) who laid down the general rules.

"(1) The hard-rock slopes of desert ranges which shed large spalls are steep, while those which shed small fragments have a low angle; (2) ranges composed of hard rock, which are thus naturally steep, maintain their steepness as long as the rock slopes endure."

Bryan (1922 and 1925) expanded these generalizations by detailed studies, classifying slopes of differing degrees of steepness and associating them with rock types which furnish fragments of suitable sizes. He pointed out, however, that, whereas the size of talus fragments (spalls) appears to control the angle of slope, the movement of material on slopes is not purely gravitational, as assumed without proof by Lawson. The pull of gravity is, of course, constant, and rocks fall and slide. Creep also occurs. The dominant process is rainwash which removes the finer products of weathering in solution and in suspension, but also carries the small rock chips and sand sizes. The abrupt change in gradient at the base of hills and mountains—the Knickpunkt of German writers—is the result of the change from unconcentrated rainwash to the more efficient flow of ephemeral streams in channels. In lands of little vegetation such as the Papago Country of Arizona, the change takes place abruptly and the Knickpunkt is sharp. In the transition into the semi-arid regions where perennial grass clothes the

slope, the zone of rainwash changes more gradually into flow in channels. The base of slopes merges with the pediment in a short, graceful curve similar to but generally less gradual than the transition curve of humid regions.

Bryan also commented on the gullying and removal of the mantle of talus of certain isolated mountains. The gullies are attributed to occasional heavy rains (1925, pp. 82–84). The possibility that these gullies might be due to overgrazing by domestic animals is not raised in the text, as many of these gullied mountains occur in areas too dry and with too little vegetation to support a grazing industry. Gullying as a result of overgrazing is more likely to occur in semi-arid rather than desert areas, *i.e.,* in the zone of perennial grass rather than the zone of the desert scrub. There is a modern tendency to attribute all gullying to overgrazing, but this is an obvious error.

That the steepness of slope of hills and mountains, inherent in the interrelations of the processes of arid erosion and the resistance of the rocks, is maintained to the end of the erosion cycle is not only asserted by Lawson but is illustrated by Davis (1932, pp. 410 *et al.*) as a specific exception to his generalization that slopes flatten with age.

The best proof that this generalization is true lies in the existence of residuals of all sizes. The typical isolated residual has a conical form and rises abruptly above the smooth and relatively low gradients of the surrounding stream-carved pediment. This residual is the *Kopje* of the South African Veld, and the *Inselberg* of German authors. Residuals when made of the same rock have the same angle of slope regardless of size. Once formed it is obvious that mere reduction in size in no way affects the processes in action. Whatever forces of degradation and whatever resistance to such forces may, in combination, have brought the residual into existence, continue to exist unmodified. The slopes must, therefore, retreat without change in angle of slope until the body of the hill is wholly consumed. But the products of weathering must be carried away from the foot of the residual. This transportation requires a gradient sufficiently high so that the available water is adequate. This relation determines the gradient of the pediment. Thus the altitude of the Knickpunkt rises continually throughout the process.

As time goes on and the residual has been consumed the low gradients of the pediment meet and a tent-shaped ridge or a low cone results. There has been much recent discussion of the form of these low cones. It is asserted that they have a convexity at the top. The maps used in illustration (Davis, 1933, pp. 241–243, Fig. 18) show no convexity but merely a continuation of the very slightly concave slopes of the flanks to the very small

summit area. Most of the argument based on this convexity appears, therefore, to lack a foundation in fact.

SLOPES IN TROPICAL REGIONS

The varieties of tropical climate are many. In fact some of the world's greatest deserts have temperatures so high as to justify the name tropical. There are also large areas with seasonal rainfall. These periodically dry tropic climates have a strong influence on the development of land forms. It is impossible here to discuss the processes on slopes in all these varieties of climate partly for lack of space but largely because of lack of data. The interesting papers by Thorbecke (1927), Jaeger (1927), Freise (1938), and others furnish an introduction to the complications of the subject.

It is proposed to treat here only the slopes developed in the tropical rain forest. These areas have high rainfalls and a short or almost no dry season. The forests consist of great trees and there is an under-vegetation so dense that much of the precipitation does not reach the ground by reason of the interference by leaves.

To many minds these areas of "rain forest" are the true tropics. The typical areas include large parts of the Congo and Amazon basins. These are broad alluvial plains or low rolling land. Our mental picture of these lowland areas is so sharp that it is hard to realize that within the tropical rain forest the mountain slopes are steep, in general much steeper than in lands having the "normal" climate of Davis.

Sapper (1935) gives an illuminating discussion of these mountainous areas and explains the existence of steep slopes in such climates. The rate of chemical decay on all types of rock is high and the products of rock decay form clayey masses on the slopes. During periods of intense and long-continued rainfall these clayey masses become saturated and highly mobile. Mud slides occur. In many places the mud flows out below the root zone. The tangle of roots binds the soil and the forest into a mat that remains in place. As the mud moves out the mat is lowered without change in position. The mud flows down the slope, and it reaches the bottom and is carried off by the streams. It may, however, form a sheet of mud on the slope, which because it is rapidly covered with vegetation becomes inconspicuous within a few weeks. In other places the slides carry the forest with them and bare rock is exposed.

The general effect of the process of land-sliding and mud-sliding is to maintain steep slopes. These slopes retreat with more or less constant declivity.

Freise (1935) believes that these phenomena are complicated in detail and has discussed them at length and calls the major process "underground

earthflow." According to him (1938) there is an intimate relation between the process of retreat of slopes and the vegetation. He contrasts this natural process with the old doctrine that "deforestation induces erosion" by the new supplementary statement that "erosion induces deforestation." This statement rests on his synthesis of a characteristic cycle of events which can be summarized as follows.

1. Bare rock slopes which on granite have declivites of 48 degrees to 52 degrees and on gneiss of 35 degrees to 38 degrees.

2. Humus formation begins through the formation of a cover of lichen and small plant growth.

3. Beginning of formation of layer of decomposed soil. Invasion by trees which, at first, are wholly leguminous in kind.

4. Mantle of decomposition on rock attains full thickness of 10 to 11 meters with development of complete tropical forest. Because earth is damp under this forest and there is an abundance of organic matter very intense chemical decomposition of the soil mantle occurs.

5. Underground earth flows and crustification of the upper soil by silicification and deposition of ferruginous minerals, with deterioration of the forest, takes place so that the soil is exposed to sunlight and tends to dry out.

6. Gradual destruction of the mantle of decomposition by rainwash and earthflow, with intrusion of grass and associated plants that replace the forest.

7. Complete stripping of the mantle of decomposition by landslide and rainwash to expose bare rock.

Thereafter the cycle begins anew. This cycle affects limited areas of the slopes at unequal rates. Thus it is possible to find within the area of the primeval forest, patches representing each phase of the process. Furthermore, there are botanical differences which make it possible to recognize the "deterioration forest" and distinguish it from the "recovery forest" or "second growth" which follows clearing of the original forest by man. The cycle is a succession of processes wholly independent of human activity. The length of the cycle is very considerable and according to Freise may reach 300,000 to 400,000 years.

In these processes or in this cycle of processes there is nothing to indicate that the slopes should flatten in time. The residuals described by Freise as Inselberge are presumably formed on the more massively jointed portions of the bedrock mass. The relatively steep slopes reported may be greater than on normally jointed rocks, but are nevertheless steeper than is common in humid regions. The existence of such residuals indicates that other residuals with somewhat gentler but still steep slopes presumably exist.

SLOPES IN HUMID REGIONS

The slopes of humid regions are in general less steep than those of either tropical or arid regions. The processes involved in the production of hill slopes are mainly rain-and-rill wash and creep operating on a soil mantle produced by weathering. This simplification of complicated processes is neatly expressed by Davis (1930, pp. 146, 147).

"In both humid and arid regions the processes of weathering are partly physical, partly chemical; more physical than chemical in arid regions, more chemical than physical in humid regions, . . . soil-creep appears to be favored and rill-wash hindered on the heavily soil-covered and forested slopes of maturely dissected mountains . . . the maturely graded slope of a soil-cloaked and forested, granitic mountain . . . is . . . about as well boulder-clad as a . . . mountain slope in an arid region . . . the boulders . . . hidden under forest cover . . . rest upon more or less decomposed bed rock . . . decomposed in mass to depths of 30, 50, or more feet . . . boulders . . . do not roll to form talus . . . but are for the most part destroyed by disintegration."

It is obvious that weathering in humid regions is a biochemical process and that there is an intimate relation between the vegetative cover and the chemical processes at work. Soil scientists differentiate at least two broad types of soil in humid regions: those in which limonite accumulates in the subsoil and those in which red oxides of iron accumulate. The first type includes the podsols of the coniferous forests of the more northerly temperate region, and the chestnut, brown, and gray earths of more southerly temperate regions. The second type are the red and yellow earths which are characteristic of the still more southerly temperature regions and in the United States extend from Maryland southward.

The concealing mantle of vegetation prevents the recognition of recurring gullying which, however, can be demonstrated in some areas. This process has been described by the writer under the term gully gravure (1940). This and other processes are doubtless in action.

Within a large part of the north temperate region, the problem of slopes is confused by reason of glaciation or of intensive frost action, dating back to the glacial period. Many observers have demonstrated that the slopes of the unglaciated Mittelgebirge of Germany were molded by this periglacial frost action. [Kessler (1925) and others.] Studies of this sort have only begun in this country, but the detailed work of Denny (1938) demonstrates that hill slopes of smooth graded aspect were developed by ancient frost action from what must have been irregular ice-contact slopes. Eargle (1940) has raised similar questions in South Carolina.

The molding of slopes by periglacial frost action occurs during periods

of ice advance and retreat in areas glaciated by a previous ice advance, and also in a zone south of the glacial boundary. How far south can only be determined by detailed study. De Lozinski (1933) points out that the preservation of periglacial features depends on the resistance to weathering of the material of which they are made. Thus block fields of quartzite may seem to be almost as fresh as if made yesterday. In schist and sandstone areas all the features such as block fields and warp may have disappeared as a result of post-glacial weathering, soil creep or rain wash.

Within the north temperate zone modern processes may be blurred by relicts of the processes of glacial time except in areas of sandstone, shale and other relatively non-resistant rock. It is thus necessary to distinguish the ancient from the modern before effective observation can be made on the processes of slope development of the temperate humid region. As in general, this distinction has not heretofore been generally made, it would seem that a new start is necessary. There is, however, a general reservation which can be made. The broader features of the land surface—its hills, valleys, and principal forms—have retained their general shape as molded by stream action, weathering and rain wash in spite of the intervention of periglacial frost action. Our neglect of this factor involves no major error in the interpretation of the successive cycles of erosion of Tertiary and Pleistocene time. It merely involves the quality of hill slopes, their inclination, continuity, and cover. Further, it has blurred our concepts as to the flattening of slopes with the progress of the cycle.

If this question of flattening be considered *a priori* what reason is there for assuming a continued flattening? If we assume that erosion has isolated a hill or small mountain 1000 feet high and 5500 feet in diameter at the base, the mean slope will be 20 degrees. Such a hill is approximately the same size and shape as Mount Monadnock which is the type residual named by Davis. Such a hill in a humid climate has essentially the same rainfall at top and bottom although the mean temperature may be lower at the top and the number of times that the air temperature crosses the frost line may be greater. If, however, it is assumed that frost action is not materially greater at the top of the hill, all the processes of weathering will be the same except those resulting from the shape of the hill.

If we assume that this hill has been reduced to a height of 500 feet, what change in process has occurred? Is there reason to believe that the slopes will be gentler? The differences in temperature between top and bottom have been reduced, but the forces of rock decay, of creep, of rain runoff—accompanied by resistance of the vegetative cover—remain the same.

These arguments are general and *a priori*. They should be supported by more detailed studies than have been yet made. Some support is af-

forded by the experiments of Wurm (1935). He subjected models formed of soft materials to rain erosion under an artificial spray. As the spray was fine, there was little erosion by impact of water drops. Reduction of the models occurred by run-off. A smooth plain of gentle slope was formed which developed from the circumferential base level toward the interior of the models. Above this "graded plain" the higher parts of all the models developed steep slopes which remained steep to the time of complete disappearance of residuals above the plain. Wurm states that the slopes become continually steeper, but his sections appear to be drawn on ridges between the minature canyons. Visual inspection of his plates indicates that the slopes of the canyon walls became steep at the beginning and that these slopes remained at esssentially the same steepness until the last residuals were consumed. In these experiments there was no resistance to rain wash by a vegetative cover and the processes of weathering, creep, frost action, etc., were inoperative. So far as they go, and apparently the experiments do adequately represent the effect of rain run-off as a separate factor, the experiments support Penck rather than Davis as to the course of slope development under stable base level.

PROBABLE PERSISTENCE OF STEEP SLOPES IN ALL CLIMATES

The foregoing incomplete review of slope development in arid, tropical and humid temperate climates indicates: (1) that slopes of hills are characteristic of the climate and the rock; (2) that these slopes once formed persist in their inclination as they retreat; (3) that they disappear only when all the volume of rock above the encroaching foot-slopes or pediments has been consumed.

If Penck's Subhaldenhang be considered the foot slope or pediment, the analogy with his Absteigende Entwicklung is perfect. It must, however, be again mentioned that under this concept the foot slope or pediment is essentially a plain cut by streams (mostly ephemeral). Penck's analysis must be in part modified, as herein suggested, before concluding that his sequence of stages is more nearly correct than those of Davis. Furthermore, it seems probable that further quantitative studies will indicate that the relative areas of foot slopes in humid regions and pediments in arid regions are influenced by climate. Thus in humid areas lateral planation and floodplain development by main streams is on *a priori* grounds, relatively easy and rapid. To the extent that coalescent flood plains are developed by man streams, the foot slopes are narrowed, and in relation to the residual the local base level is continually lower and brought closer. In arid regions the main rivers are relatively feeble unless supplied by water from sources outside the area. In consequence the area of pediments is relatively large and in relation to residuals, the local base level is relatively stable and

remote.

The argument here given rests far too much on generalities and is too little suggested by detailed studies. Many climatic types, each having a characteristic combination of processes, each with a characteristic vegetative cover, cannot as yet be analyzed. Even in the main climatic zones our detailed knowledge of process and resulting land form is meagre. More detailed and quantitative studies on the influence of the rock type on slope are urgently needed. It is to be hoped that the challenge of Penck's analysis, obviously at fault in many particulars, will nevertheless be a stimulus to renewed efforts in the study of the land forms of the several climatic zones of the earth.

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Chairman Birdseye: I think we will move on to the two final papers without discussion at this time. *Prof. O. D. von Engeln* of Cornell University will present: "A Particular Case of Knickpunkte."

A PARTICULAR CASE OF KNICKPUNKTE

A graded stream has a smooth, continuous profile. The profile of an ungraded stream may be made up of slopes ranging all the way from the vertical to the nearly horizontal in degree of declivity. Between these extremes are streams that have graded reaches separated by sharply defined descents of waterfall or of cascade steepness. Such interruptions to other-

wise smooth stream profiles are designated Knickpunkte in Walther Penck's lexicon.

Where a stream is actively downcutting in a structure comprised of horizontal or nearly horizontal beds of sedimentary rock the occurrence of Knickpunkte, as defined above, in its profile, as got from a contoured map, will, in orthodox geomorphology, be attributed to cap-rock strongly resistant to erosion underlaid by a considerable thickness of weak beds. If the profile is interrupted by a succession of such Knickpunkte, with graded reaches between, a number of repetitions of the strong-weak alternation of beds is assumed to be a characteristic of the vertical sections of the landmass structure. The graded reaches, then, are developed on and slightly above the top surfaces of the falls-forming strong layers.

If such relations are not obviously discernible when the sites are visited indoctrination leads to the inference that the structural circumstances deemed essential for the development are present but are not apprehended by the observer. The other possibility, which Penck upholds, that such Knickpunkte, separated by completely graded reaches, develop in perfectly homogeneous rock, such as a massive intrusive body, has not been given consideration. It is Penck's contention, further, that the Knickpunkte need not be initiated by any degree of intermittence in diastrophic or eustatic movement.

The concern here is not with this latter contention. Even if it can be satisfactorily refuted there remains the question: Can a Knickpunkt, initiated by any type of original, *i.e.*, consequent, sharp declivity in a stream profile be maintained (or perhaps even accentuated and enhanced) when such variations in structural resistance as give rise to cap-rock waterfalls are not present. The phenomena exhibited in the gorge of Cascadilla Creek at Ithaca, New York, appear to demonstrate that this is not only possible, but is also a normal result.

As is well known the Finger Lakes valleys of central New York State were glacially overdeepened in the measure of 500 to 1000 feet. In consequence of this overdeepening tributary valleys were left hanging. Cascadilla Creek is on one of these hanging valleys. During the period of melting back of the last glacial advance proglacial lakes were temporarily maintained in the main valleys, at altitudes up to 500 feet above the present lake levels, by the ice barrier. As the ice front became established along lines successively farther north, lower and lower outflow channels were made available to the lake waters. This permitted sudden drops in the proglacial lake levels. At each such drop a new, consequent, declivity was introduced in the stream profiles of the hanging valleys by descent across a lower section of the oversteepened valley side of the glacially excavated troughs of

the lake-occupied main valleys. The first such water-fall descent developed farthest upstream in the tributary valley and was followed, in turn, by others farther and farther down stream. It seems that some, at least, of the proglacial lake levels existed over considerable periods of time, as is indicated by the volume of the deltaic deposits at the tributary stream ends at appropriate altitudes.

The orthodox expectancy for the consequent waterfalls, thus initiated, will depend on the structural circumstances. If marked alternations of resistance of beds in the vertical section is the situation, each fall should be eroded down to the level of the top surface of a strong cap-rock layer, and then experience an upstream retreat of its crest line. At the same time the reach below the retreating crest should be graded with reference either to the lake altitude as a base level, or to the next lower layer strong enough to function as a cap rock, and having an elevation above the lake base level. If, on the other hand, the layers in the vertical section subject to erosion are of essentially the same degree of resistance to erosion, the consequent falls should be eliminated at the site of their initiation by groove notching of their crests.

Contrary to such expectation the falls in Cascadilla Creek appear to have been preserved and perhaps even enhanced in height without benefit of significant differences in resistance of the beds affected by the erosion of the gorge. Between the time of a higher lake level and the establishment of the next lower one, the fall, Knickpunkt, first developed, seems to have retreated an appreciable distance upstream, without loss either of height or of steepness. The falls behaved as cap-rock falls should but were without cap-rock structure. Further, they have persisted in this habit until now. At the brink of no one of the falls can a strong cap rock be discerned. The reaches below the falls are well graded but not over, or on, a distinctively resistant bed. The falls are veritable Knickpunkte of Penckian type. Examination of the walls of the gorge also fails to disclose any sufficient variation in rock strength to warrant the development and maintenance of cap-rock falls of the height and sharpness of those actually present. Slightly stronger layers do give rise to low step falls, as is appropriate, but these occur both in the faces of the larger falls and in the reaches below the big descents.

The four pictures[7] reproduced at the end of the symposium permit a visual appreciation of the circumstances. The significant items are (1) the irregularity of bedding and general uniformity of the vertical rock section, (2) the height and number of the Knickpunkte falls, (3) the graded condition of the reaches below them.

It remains to be discovered whether any correlation can be established

[7] Twelve lantern slides were shown when the paper was presented.

between the altitude and number of the Knickpunkte falls and the temporary, proglacial, lake levels. Examination of other gorges, of which there are a number in the region, will be significant in this study. Distinctively cap-rock falls do occur. Are both Knickpunkte falls and cap-rock falls to be found in the same stream profile? In Cascadilla Creek jointing of the rock, transverse to the gorge course, is very pronounced. Is this jointing a factor of significance in the preservation of the Knickpunkte? Answers to these questions are being sought by students engaged in graduate study at Cornell University under direction of the writer.

Chairman Birdseye: The final paper was prepared by Mr. Walter Hacker of San Francisco State College; but in the absence of Mr. Hacker Professor Leighly is going to read the paper.

Prof. John B. Leighly, University of California: This paper of Hacker's is little more than a footnote to Penck; it constitutes a correction of one of Penck's ideas that has been taken up and used by various authors who have followed Penck.

Prof. Leighly read the following paper prepared by *Walter Hacker.*

OVERLOADING AS A MOTOR OF MASS-MOVEMENT

According to Albrecht Penck's formulation,[8] a rock mass of weight g which has its smallest cohesion with the underlying rock along a surface of area F that forms an angle α with the horizontal will remain at rest as long as the downward pull parallel to the surface F is smaller than the sum of friction and cohesion; or,

$$g \sin \alpha < g \cos \alpha\rho + F\, c, \qquad \ldots\ldots\ldots\ldots \text{ I)},$$

where $g \sin \alpha$ is the downward pull, $g \cos \alpha$ the component of g perpendicular to F, ρ the coefficient of sliding friction, $g \cos \alpha\rho$ the amount of friction, c the cohesion per unit of area. If cohesion is reduced to zero, the rock mass will be at rest as long as:

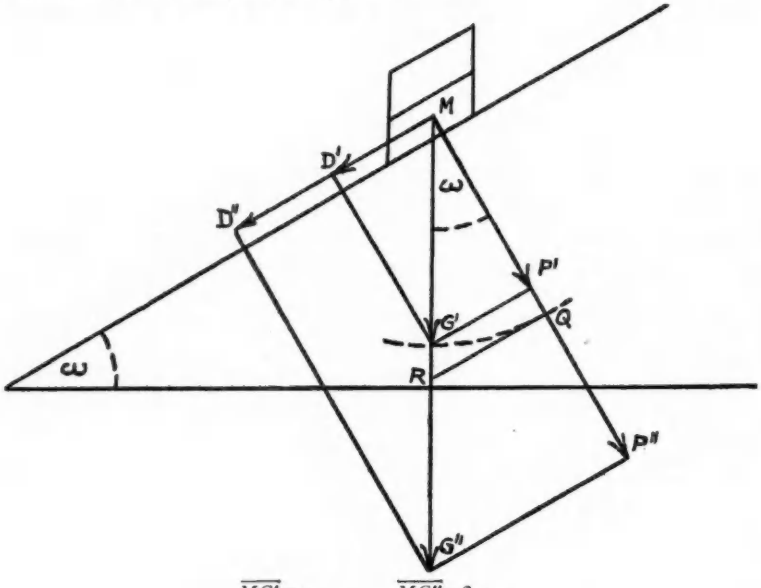
$$g \sin \alpha < g \cos \alpha\rho \qquad \ldots\ldots\ldots\ldots \text{ II)}.$$

Provided that the coefficient of friction remains constant, an increasing inclination of the supporting surface will increase the downward pull at the expense of the component perpendicular to F, until the angle of inclination is so great that its sine equals the product of its cosine and the coefficient of friction. If the coefficient of friction is 1, the angle of equilibrium will be 45°, since the sine and the cosine of 45° are equal. If ω is the greatest possible angle with which a certain rock mass will still be at rest, then:

$$g \sin \omega = g \cos \omega\rho,$$
$$\text{and } \rho = \tan \omega \qquad \ldots\ldots\ldots\ldots \text{ III)}.$$

[8] Albrecht Penck: *Morphologie der Erdoberfläche.* Stuttgart, 1894, pp. 222 ff.

Therefore, if cohesion has been reduced to zero, a rock mass will be at rest if $\alpha < \omega$; downward pull and friction will balance each other if $\alpha = \omega$; the rock mass will move downward if $\alpha > \omega$, provided that there is no obstacle in its way. In general, no natural obstacle will be in its way if the inclination of the valley slope is greater than α.



$\overline{MG'} = g$	$\overline{MG''} = 2g$
$\overline{MD'} = g \sin \omega$	$\overline{MD''} = 2g \sin \omega$
$\overline{MP'} = g \cos \omega$	$\overline{MP''} = 2g \cos \omega$
if $g = 1$:	$\overline{RQ} = tg\, \omega$

The four factors determining the stability of a rock mass are, therefore, the inclination of the surface of smallest cohesion, the cohesion, the coefficient of friction, and the inclination of the slope. The weight of the rock mass does not enter the equation as a determining factor, since both downward pull and friction increase at the same rate with the weight of the rock mass.

There would be no need to state this fact again if it were not that several authors, notably Walther Penck, Erich Obst, C. F. Stewart Sharpe, and Philip Worcester,[9] have arrived at different conclusions.

[9] Walther Penck: *Die Morphologische Analyse.* Stuttgart, 1924.
Erich Obst, in A. Supan: *Grundzüge der Physischen Erdkunde.* Berlin, 1930.
C. F. Stewart Sharpe: *Landslides and Related Phenomena.* New York, 1938.
Philip G. Worcester: *A Textbook of Geomorphology.* New York, 1939.

In the fourth chapter of his *Morphologische Analyse* Walther Penck deals with mass-movements. After discussing the resistance of rock masses against the pull of gravity by cohesion, friction, and anchoring by plant roots, he examines the factors which may release a mass-movement. He calls these factors motors of mass-movements. He comes to the conclusion that there exist two classes of motors: first, increase of weight (*Gewichtszunahme*) and second, changes in volume. It is the first, the increase of weight, or the process of "overloading" as it has been named by English authors, with which we are concerned here.

To state his case, Walther Penck describes an observation which he made during the eruption of Mt. Vesuvius in 1906. He says, in translation, on page 68: ". . . The material (ejected by the volcano) consisted of a loose mixture of volcanic fragments, lapilli, sand, and very fine dust, and possessed, therefore, an extraordinary mobility. This mobility, further increased by a reduction of friction because of the considerable temperature of the ejected material, found expression in the liveliness of the downward movement. The masses, however, did not begin their movement instantly after they had been deposited, but only after they had reached a certain thickness. Once in motion the masses came to a halt only at the foot of the mountain where the inclination of the slope decreases very rapidly. This proves that gravity, although causing the movement and sustaining it as long as its component, which decreases with the decreasing inclination of the slope, is greater than friction, is not in itself sufficient to initiate such motion. In other words, gravity is not sufficient to overcome the resistances which occur at the beginning of the movement, above all the friction (momentum of inertia) which is especially great at this initial stage. All those factors which bring about the release of a downward movement of masses which have been previously prepared for it or which promote it may be called motors of mass-movements. In the case of the cinder avalanches of Mt. Vesuvius the motor was the increase of load (Gewichtszunahme)." Thus, whereas the author first emphasizes that gravity as such cannot release a mass-movement, he makes thereafter a mere increase of gravity responsible for such a release. To support his argument the author offers, on page 69, Albrecht Penck's mathematical formula[10] cited above. However, instead of concluding from it that changes in the weight of the rock mass alone cannot start a mass-movement, W. Penck declares: "From this it results that for every weight there is a certain greatest inclination of the supporting surface with which it will just remain at rest, and this angle is the greater the smaller the weight of the rock mass." And again, on the same page: "If a mass which is resting upon a surface of the greatest pos-

[10] The use of the letter "a" in the left side of the equation instead of "α" as in the right side is a misprint according to the explanation of the symbols given in the text.

sible inclination experiences an increase in weight, then this increase in weight will *under all circumstances* release a downward movement." The same idea is then repeated in other connections, and in note 118, page 255, the author states once more: "The smallest inclination of a surface, along which a downward movement can start after cohesion is reduced to zero, is the inclination of the *Reibungswinkel;* it is smaller the greater the weight of the overlying rock mass is."

Walther Penck discusses, hereafter, in detail the effect of moistening the regolith or rock masses rich in colloidal matter. Here too he fails to point out the effect of the decreasing coefficient of friction and tries to explain the occurrence of slides, slumps, or earthflows as a direct result of the increase in weight through absorption of water. Also here he comes to the conclusion (page 71): "The increase of weight through absorption of water is beyond doubt a motor, the importance of which upon mass-movements cannot easily be overestimated."

If a mass-movement actually occurs after an increase of weight has taken place, it can happen only because one or more of the determining factors has been changed. With cohesion being reduced to zero, the inclination of the slide-surface and of the slope remaining unchanged, the only variable factor left is the coefficient of friction. It is beyond doubt that in many instances an increase of the load will bring about a reduction of the coefficient of friction. Under certain circumstances such an increase will rearrange the axes of the rock particles in such a way that they will make an angle with the supporting surface smaller than before. It will largely depend upon the kind of rock whether such a rearrangement of the particles can be brought about, and under what minimum pressure. But a diminution of the coefficient of friction is not a necessary result of every increase of weight; there are cases where an increase in weight might even increase the coefficient of friction. Under certain conditions an increase of weight might make a rock mass more compact and thus more resistant to changes that diminish friction. It might result, for example, in a more effective interlocking of the rock particles. It might narrow and even close cracks and fissures and thus prohibit the circulation of water that would act as a lubricant. It might reduce the ability of rock particles, especially of colloidal matter, to absorb water and make it thus less plastic. Let it be assumed, however, that in certain instances an increase of weight tends to reduce the coefficient of friction and is followed by a mass-movement. Then the mass-movement will start after the necessary rearrangement of the rock particles along the sliding surface has been completed. This process will require a certain time, and after it has been accomplished the mass-movement will occur suddenly. If the increase of weight were directly responsible for the mass-movement, as

Walther Penck insists, then every additional increase of load would result in a gradual diminution of the inclination of the supporting surface and find its expression in immediate mass-movements. If the weight were the determining factor alone and by itself, cones of different volumes of piled up sand, gravel, or other loose materials would have different maximal slopes, contradictory to our experience.

In the case of moistening the regolith or loose rock masses, the coefficient of friction is again the sole determining factor. The coefficient of friction might be decreased because the absorbed water represents an increase in load and causes a rearrangement of the rock particles or by the lubricating effect of the moisture upon the surface of least cohesion or by the combined action of both factors. After a mountainside has been covered with snow, it might very well be that mass-movements would not start at the time when the snow cover reached its greatest thickness and the additional load therefore greatest, but later, even when a part of the snow was already melted and lost by run-off, if the remaining part, were absorbed into the ground, moistened it and so lubricated the potential slide surface.

From the quoted passages, however, it follows clearly that Walther Penck held no such changes in the coefficient of friction responsible for the release of mass-movements, but the weight of the overlying rock mass alone and as a directly acting factor.

Upon the same concept Erich Obst bases his discussion of mass-movements. He speaks in this connection (*op. cit.*, p. 164 ff.) of a "movement-producing pressure (*Bewegungs-erzeugender Druck*)" and says, in translation: "Above all, this problem has to be settled: Of what importance is an increase of weight such as, for instance, is brought about by an increase of the amount of weathered material? It is certain that by such an increase a mass-movement is often released, where before, under the given inclination of the slope (better: the surface of support), the *pressure was not sufficient to overcome the friction.*" Since the author does not consider the coefficient of friction which might be changed with changing pressure, this would imply that a mere increase of pressure would at a certain point be able to counterbalance friction. We have, however, seen at the outset that friction increases at the same rate as the pressure. That this fact is known to the author is shown by the following statement, made a few lines beyond the one cited above: "On the other hand, it is certain that increase of pressure increases the friction." It is the failure to consider the coefficient of friction as the determining factor which makes the question problematical.

That C. F. Stewart Sharpe also has the same misleading concept with regard to the effect of an increase of weight is shown by the fact that he introduces as "Active or initiating causes" of mass-movements (*op. cit.*, p.

84 ff.) besides a group named: "C. Reduction of friction through: . . . ," a group: "B. Overloading through: . . ." This indicates that the author considers "overloading," an increase in weight, by itself, as a motor of mass-movements and not as a mere cause for reduction of the coefficient of friction with which he deals under "C."

"Overloading," besides lubrication and over-steepening, appears also in Philip G. Worcester's book (*op. cit.*, pp. 422 ff.) as an active cause of landslides. In this instance, too, it is indicated that the author accepts a direct influence of an increase in weight and not a change in the coefficient of friction brought about by it. The author thus follows C. F. Stewart Sharpe, whom he quotes.

A. K. Lobeck[11] avoids the issue by quoting C. F. Stewart Sharpe only in part (p. 93) and by omitting a discussion of the problem involved.

Summarizing, it may be restated that while an increase in weight might in many cases release a mass-movement, this result would follow only when it succeeded in changing the coefficient of friction. The concept that an increase of weight might by itself, directly and without a change of the coefficient of friction, release a mass-movement is contradictory to an established law of mechanics.

DISCUSSION

Chairman Birdseye: We have had some very interesting papers presented here this afternoon and are now ready for a period of discussion of them.

President W. W. Atwood: Mr. Chairman: This has been a remarkably interesting program, but much has been left unsaid. I should be very remiss, especially while we are guests of this university, if I did not express appreciation and indebtedness for training in geomorphology received from several members of the faculty of this institution. It is my duty to emphasize at once that for over thirty years no one trained here in geomorphology failed to learn that as the land went up erosion continued. There is nothing new in that concept. It has appeared in the English language over and over again in the last fifty years. I should be remiss again if I did not tell you that right here in this university, and forty years ago, I was told by the most profound student of the earth sciences this country has ever produced, that one of the greatest contributions geomorphology could make to science was through the light it might throw upon diastrophism. When about to leave for the San Juan Mountains in 1909 Dr. T. C. Chamberlin, then chairman of the Department of Geology in this university, called me into his

[11] A. K. Lobeck: *Geomorphology.* New York, 1939.

office and emphasized the many ways I might find in the study of the physical history of mountain ranges, for discovering the movements that had taken place in the crustal portion of the earth. There is nothing new to American geomorphologists in that idea.

I believe, Ladies and Gentlemen, if this paper by Dr. Walther Penck had been so written that anyone could translate it into clear English, we should never have had much trouble in harmonizing the author's ideas with those that have been commonly known in this country.

Allow me to turn to two or three experiences that bear upon this problem. In the study of the crest-line peaks of the Rocky Mountain ranges we find little or no evidence in these high regions of long stationary periods. We have emphasized over and over again in our reports that the mountains appear to be growing today, and we have said in just about these words that "they may be growing as fast today as they have at any earlier time in their history." We have worked for decades with the hypothesis of erosion proceeding as the mountains were being uplifted. Nevertheless, we do find in the long record, which extends through millions of years, that there is evidence of one period when the mountain area stood relatively stationary for a long time. During that period the one remarkable peneplain recorded in the Rocky Mountain region was developed. No student of geomorphology, trained in any country in this world, should be unable to recognize that surface if he is in the field among the Rocky Mountains of our country. Similar evidence of long stationary periods is recorded in the landscape in many other parts of the earth's surface.

Evidence has been presented many times of the uplift of the Front Range of Colorado while the Arkansas River has been cutting the Royal Gorge. During certain periods the uplift was so rapid that the stream was ponded and a great lake formed to the westward in the vicinity of Salida. Then the uplift became less rapid or the downward cutting was accelerated and the lake was drained. It is reasonable to believe, from the evidence in that part of Colorado, that there are periods of uplift and periods of stability. The paper on Rocky Mountain Ranges of Colorado which we listened to earlier today has emphasized many of these same facts.

About a week ago I listened to a dozen or more specialists in geology, geodesy, and seismology who came together at the American Academy of Arts and Sciences in Boston to discuss the nature and composition of the interior of the earth and the movements which occur in the crustal portion of this planet. We were told positively that the crustal portion has a thickness of about forty miles, and that it is resting upon a fluid zone with virtually no stability. Evidence was presented from recent studies made in the Fenno-Scandia area of northwestern Europe of a depression, due to the

presence of a continental ice cap, of at least 2,000 feet. Evidence was also presented of the rising in the center of that great bowl of 1,400 feet since the close of the ice age. The uplift must amount to 600 feet more to bring that part of the crustal portion of the earth into the position in which it stood before the great ice sheet of the Pleistocene period formed.

Evidence was also presented of the depression of the Hudson Bay region in the Great Laurentian Shield, and the subsequent uplift since the disappearance of the ice sheets in this continent, of about 900 feet. That movement is still going on and is measurable. In the discussion which followed the presentation of several papers at that meeting I took pains, because I knew I was coming to this meeting, to question the speaker critically on the nature of the uplift in each of the areas where recent movements in the crustal portion of the earth are clearly recorded. In that discussion it became clear that the most significant evidence is found in certain shorelines that are etched upon the lands bordering the Baltic Sea and upon the shores of the Great Lakes and Hudson Bay. Each shoreline represents a period of relative stability and the bending of those shorelines represents a period of movement.

I come back again and again to one of the fundamental elements in the philosophy of Dr. T. C. Chamberlin, that the crustal portion of this earth, because of its strength and rigidity, will hold firm for a period, but as stresses and strains accumulate there will come a time when some movement will take place in order to relieve those stresses and strains. That means there are periods of stability and periods of movement. This I think is clearly demonstrated in the field work of many geologists and geomorphologists.

We can welcome the emphasis which Dr. Walther Penck has placed upon the nature of erosion in homogeneous material during periods of continuous uplift. I think our modern philosophy of geomorphology should provide for an understanding of those conditions in other parts. There is variety in the conditions in the different parts of this planet and we must provide for variety in our philosophy of geomorphology.

Chairman Birdseye: I don't want you to think, Dr. Atwood, I know anything about geomorphology. I am in a peculiar position in trying to control the discussion which I know is still going to follow.

Prof. Meyerhoff: Dr. von Engeln's remarks on Knickpunkte are very significant, and I trust we may ultimately learn the results achieved by the students he has working. I have made a study of one set of Knickpunkte in New England, where, in spite of glaciation, pre-glacial breaks in stream gradients can be identified and can be related to the inauguration of a new erosion cycle by uplift. In this set of knickpoints we have found that ap-

proximately thirty-one out of thirty-four streams break at constant elevations, variable within the normal limits of the stream gradient. And we find the variability in elevation and in distance from stream mouth exhibiting the regularity of a natural law. Crudely stated, the law might be phrased as follows: the larger the stream, the lower the knickpoint, and the farther its position upstream from the mouth. In the case studied, the streams occupy every conceivable relationship to lithology and structure, and it affects mainstream and tributaries in the same drainage system, as well as members of different systems. In the research of which Dr. von Engeln has told us, it would be instructive to learn whether the lengths of level reaches in the several streams can be related to water volume.

Dr. W. W. Atwood, Jr., Clark University: Mr. Chairman: My personal opinion is that the Penck and Davis theories do not differ greatly. There is much to be said in favor of each of these interpretations of geomorphic history. In the Rocky Mountain region where father and I have been working during the past few years, most of the erosional features can be explained by the processes described by W. Penck. However, we are inclined to believe that there have been definite periods of stability during which the Rocky Mountain Peneplain and other erosional surfaces have been developed. I wish to comment on what the Chairman said about preparation of these papers. This sort of argument runs into words because if you go into real study of all the postulates, there is a book full of them. You get into difficulties which Hacker got into in his paper, which Professor Leighly read. He made any number of assumptions. One of the assumptions which he made was that slope retreat can be cut in thin slices by strictly gravitational means, slopes being made by weather. This is obviously erroneous. We have too many other ways and processes of reducing slopes. By this assumption he tended to oversimplification. If you make it oversimple you reduce it almost to a mathematical level. One of the reasons mathematics and history cannot get along is that mathematics makes too many assumptions. I urge you all to study Penck's system of retreating slopes, what he calls slope association of various forms.

Prof. C. C. Colby, University of Chicago: I did not wish to speak because I am afraid I shall talk off the point. Years ago I tried to observe evidences of peneplains in the field, and I always found that there were land forms and associated features in the landscape which were not mentioned in articles dealing with the phenomena. The theories, and the articles written under them, failed to satisfy my curiosity. As time has gone on, I have found that increasingly true. Evidence of landscape features which have not been adequately discussed in the southern Appalachians, for example, are so numerous that I think the evidence of peneplains in that chal-

lenging area should be re-examined. When we come to the question of slopes and the other ideas which Professor Bryan has presented, I am greatly interested. We who are studying land forms not for origin primarily but for their relation to human activities are trying to approach the matter of slopes from a quantitative standpoint—that is, we are trying to map the distribution of the slopes according to classes. The work is going forward satisfactorily and perhaps eventually we may be able to turn over to the physiographers useful statistical material on the areal repetition of slope forms.

Chairman Birdseye: Dr. von Engeln, don't you think you ought to wind up this symposium that you have arranged?

Prof. von Engeln: That's very courteous but I don't know that I can add anything especially significant. I'd like to assure Dr. de Terra that he is not to be regarded as a victim. The victims are the graduate students that Professors Meyerhoff, Cressey, and others have pulled into this business to do the Penck translation.

With regard to President Atwood's remark that because Penck wrote in German he gets greater attention than he would otherwise, I'm not sure that's quite fair. He did write in German and wrote very tortured German. I have been putting out the theory that it is because he was seriously ill and physically distressed that he wrote such very involved sentences. That is considered a natural trait of German but Penck's father writes a fluent and easy German.

As Professor Bryan suggested, Walther Penck's book is packed with suggestions and facts and includes every qualification that he could think of. One needs to ponder it line by line, simply to see what Penck meant. It is regrettable that we had so little time for discussion. I felt that for my part I had made a contribution in formulating the propositions which I'll say indicated not so much final conclusions as questions to be answered and were intended to stir up maximal reaction on the part of those who might participate. They'll stand in the record and I think the Secretary has some idea of getting the symposium as a whole into print. Then we can go on at some future time and in due course find out what it is all about.

The photographs (Figs. 1–4) which appear on the following pages illustrate the paper "A Particular Case of Knickpunkte, pp. 268–71.

FIG. 1.—Cascadilla Gorge, Ithaca, N. Y. Upper section. General view to show succession of falls and reaches. Descents at falls range from 20 to 50 feet. Reaches are several hundred to a thousand feet long. Oct. 31, 1939.

FIG. 2.—Characteristic Knickpunkt fall in Cascadilla Gorge, Ithaca, N. Y. Second one down in Fig. 1. Note absence of any marked cap-rock bed in fall's face. Oct. 31, 1939.

FIG. 3.—North wall of Cascadilla Gorge, Ithaca, N. Y. at and below crest of fall shown in Fig. 2. Note absence of any bed of superior resistance in the section. Nov. 18, 1939.

FIG. 4.—Characteristic reach with nearly flat floor, all on rock, from crest of fall terminating reach in foreground of Fig. 1. Also note that lower, downstream, portion of wall of gorge similarly devoid of fall-forming beds. Nov. 18, 1939.